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THE PLACE OF THE ECONOMIST IN GOVERNMENT[1]

[Given by SIR ROBERT HALL *before the University of Oxford on 5 November 1954]*

WE are here to commemorate the work of Sidney Ball, Fellow of St. John's College, and especially his contribution to the advance of social studies in Oxford. He died before the Honour School of Philosophy, Politics, and Economics was established, but he would have been gratified by its steady growth for more than 30 years. During this period economics has become an increasingly specialized subject, and it is this development which is largely the occasion of my lecture today.

You will understand that it will be a personal view. But although there are divergent opinions about the place which the economist ought to take in the machinery of government, there would be substantial agreement that a modern government is very closely involved in the economic life of the country. The economist himself claims as his province the description and explanation of the economic system, which covers the whole process of producing and distributing the national income. In the United Kingdom at least one-third of this passes through the hands of the government. It is obvious that no system could be found which would make the collection and spending of such large sums an economically neutral activity. In the United States the proportion is about one-fifth. In the U.S.S.R. it is very much higher. The economic activities of the modern State are very large in relation to total economic activity.

On a qualitative view, it is also self-evident that the growth of the share of the national income taken by the State has been associated with a comparable growth in function. The familiar story of the past hundred and fifty years has traced for every student of modern history the process of change. In quite recent times the State sought to carry out a minimum of economic responsibilities; today it has a finger in every pie, and often both hands.

There are all the activities connected with maintaining what has been called the national minimum—education, the health services, the conditions of employment, unemployment insurance, pensions. There is direct participation in the process of production. In the United Kingdom the State owns the coal-mines, a good deal of the transport, and nearly all the communications; it supplies electricity and gas, it has much to do with

[1] *The Sidney Ball Lecture, 1954.*

water, it determines the pattern and settles the income from farming; it also owns the Bank of England.

As we leave the basic industries the degree of control becomes less direct, although much of the pattern of the engineering industries is determined by our expenditure on defence. It is true that the very extensive and detailed controls necessitated by the war are now passing rapidly away, but every branch of industry still has a government department to which it looks for guidance and often for help. There are constant contacts between industry and departments, which cover a multitude of subjects.

The extent to which the government ought to participate directly in the affairs of industry is, of course, one of the great controversial questions of our time, and fortunately not one with which I have to concern myself today. The facts of its present participation are clear.

But quite apart from these particular activities, there are still the general and, I think, more fundamental responsibilities of government, those which determine the framework and the climate in which all economic activity takes place. The most important of these is the regulation of the general level of economic activity, which determines the extent to which resources are used, and especially the levels of employment and of profits. The acceptance of responsibility for the level of activity, which is common to all political parties today, is the biggest single change in this field since the pre-war period. Before 1939 no British and few other governments accepted responsibility even for the level of employment, and at any rate until the 1930's it was not considered that it was within the power of the government to determine this level.

This is a field in which there is still much to learn, as to exactly what a government can or cannot do. It seems fairly safe to say, however, that modern governments will expect, and will be expected, to do their best to maintain acceptable levels of employment. This in turn means that the government, at least in countries where foreign trade is important, has to watch and to find means of affecting, the foreign balance of the country, since it cannot use changes in total activity as a balancing factor. It also means that the government has to exercise some control over the level of investment; and, in fact, many governments, including our own, feel that they ought to choose policies which lead to what can be loosely called a high rate of investment. They do this not only because of the connexion with the maintenance of high employment, but because investment leads to the growth of the national income.

Thus governments, now and in the future, will have to concern themselves with budgetary and monetary policy because of their effects on employment; with the foreign balance and with investment because these are elements in the determination of the level of activity; and with

investment policy, since it is the tendency in nearly all modern States to regard a steady increase in national income as a proper object of policy.

It has long been the case that the State has had to concern itself with matters of foreign trade, the negotiation of treaties of commerce, questions of tariffs, shipping, and so on. It is usually considered also that the State has a duty to determine the conditions under which monopolistic practices shall be allowed. There is the wide field of industrial legislation, which lays down common conditions under which employment and production must take place. And there is the question of industrial relations and the extent to which the government should concern itself with strikes and lock-outs.

All these economic functions are reflected in the growth of the great Departments of State which are primarily concerned with economic matters. The Treasury and the Board of Trade are predominantly economic Ministries, and the Ministry of Labour is to a large extent. The Ministries of Agriculture and Fisheries, Fuel and Power, Supply, and Works are all concerned with particular groups of industry, either with trading functions or with special supervisory responsibilities. The Post Office is almost entirely a trading department. The Foreign Office, the Commonwealth Relations Office, and the Colonial Office, along with the Commercial Relations and Exports Department of the Board of Trade, are compelled to interest themselves in external economic relations. And the work of many other departments, for particular matters of policy or because of their control of expenditure, has economic aspects in varying degrees.

It would be labouring the obvious to continue in this way. In the Sidney Ball Lecture for 1924—'The End of Laissez Faire'—Maynard Keynes called attention to the tendency of the times and the tendency has continued. The modern British Government, and a great many other modern governments, have, and seem likely to continue to have, very extensive economic functions.

It would be a very simple-minded view, however, to hold that modern governments ought to employ large numbers of economists. A large part of the population spends a large part of its working life in activities which are primarily economic, that is, which are directed towards the production of economic goods. In simple terms, most of us work for our living. But we get along very well without any help from economists, and there is a strong tradition in English economic thought, that one of the essential functions of economists is to point out that the less the government interferes in economic matters the better.

This is, I fear, a lost cause. But most people would agree that economists are no more successful than anyone else at earning their living or running

a business. There is no more and no less presumption than in the case of private business that the government should employ economists because it is engaged in business activities.

I myself think that there is a place for the economist in really large enterprises, such as those conducted by governments. But I do not think that it is necessary or important that those people engaged in the ordinary work, and most of the supervision, of economic activities, within or outside the government, should be economists. Nor is this necessary for most government officials engaged in work which is connected with economic activities. Those who are engaged in business, or in business-like work, need to understand the general principles of business and to have that general common sense which enables them to learn about the particular work they are doing, in any walk of life, and to carry it out efficiently.

Most activities which the economist would class as economic do not require a training in economics, and such a training is mainly helpful to the extent that it gives, or is part of, a good general education. Economics is an aspect of most human activity, but is not the activity itself. An engineer or a chemist needs a technical training: if he rises to direct such enterprises, he will need a layman's knowledge of other subjects, such as law and accountancy. At all stages he will be very much helped by an administrative sense and the ability to deal with other men, skills which can only be taught to a limited extent. But he might never need the specialized training of a professional economist.

In the same way, the civil servant in an economic Ministry will need a good general education so that he can be technically proficient in dealing with what is inevitably largely paper work. As he progresses he will need specialized knowledge of the subjects with which his department deals. And at all stages *he* will be helped by administrative sense and the ability to deal with other men. But he also may never require the special training of the professional economist. Both the successful business man and the higher civil servant in an economic department will inevitably become conversant with some parts of the economist's ways of thought, just as they will need to become conversant with parts of the thinking of other specialists. My general point is that there is no obvious reason why most of those who are engaged in activities within the province of the economist should themselves be professional economists.

Most businesses, however, do need to employ professional men to advise them on technical matters, and this would not be questioned in the fields of science, or of law or accountancy. The economist is trained to think about changes in prices as instruments of action, ways of bringing about given results. He also has a special training in connexion with those functions which involve the economic system as a whole.

It is, of course, a commonplace among economists, as among philosophers, that everything involves everything else. This was illustrated in Marshall's homely simile of the balls in the bowl, where all would move if one were touched. The modern economist expresses the same thing in a system of simultaneous equations which cover the whole economic field. For practical purposes it is usually quite enough to consider problems as if they were isolated, and to bear in mind only the general maxims which are the foundation of everyday life, such as that in business it is desirable to buy cheap and to sell dear, or in government that one must be very careful in creating precedents.

But there are a number of problems in which common sense breaks down and offers no guide at all, because the subject-matter is too complex to be grasped without training in a special discipline. In economics, the most important example is in the field of national income analysis, which forms the basis of the theory and the practical application of a policy of high and stable employment; and which constitutes the foundation of a modern Budget, in so far as the Budget is used as an instrument to maintain the balance of the economic system. Similar methods are employed in other problems, such as those connected with the balance of payments. The calculations involved are undoubtedly of sufficient complexity to require the specialized skill of a professional economist-statistician; without his assistance they could hardly be done at all, and it is extremely difficult for the layman to find his way among the problems involved in the changes in any part of the system, without a professional guide.

This is equally true in the other great field which influences the whole climate of economic life, that of central banking. The influence of monetary policy is felt throughout the whole system and is of the first importance. But the nature of its operation is extremely complex and difficult to understand, although monetary theory, unlike national income calculations, seems to have an irresistible attraction for the amateur and the crank. Central bankers might perhaps class a good many economists with one or other of these. But there can be no doubt that a central banker is concerned with operations in the economic field which are directed towards general results and are not concerned with the straightforward calculations of profit and loss which are made by the business man, and that these operations require a special expertise. It is therefore necessary for the government to have professional assistance in assessing those questions in the monetary field which involve the responsibility of government. In fact, the whole complex of the level and balance of economic activity is, and should be regarded as, a single problem requiring a simultaneous solution. It is for this reason that it has become customary to speak of monetary-and-fiscal measures as if the adjectives were hyphenated. They

refer to the whole process of operating on the economic climate by general, as distinct from detailed, measures.

There are a number of other activities, in which modern governments are engaged, which have similar characteristics; that is, which involve to a significant extent the totality of economic forces, and which cannot therefore be judged by any simple or common-sense rules, such as the cost or the profitability of the immediate transaction.

The traditional example in this field is that of external economic policy. Take tariffs, for example—if one could look at a tariff on a single article in isolation, it seems a matter of common sense, though it may not always be admitted, that a protective tariff benefits the industry concerned and raises the price of the article to the consumer. As a practical approximation no great harm is done if we do not try to look any deeper.

But when a number of tariffs are involved, the effects on employment and exports have to be considered. Even more, when we live in a world where everything we do may affect what is done by other countries, either through the complexities of the General Agreement on Tariffs and Trade; or the even greater complexities of bilateral trade or payments agreements, the subject-matter becomes extremely difficult. The different strands and interactions involved ramify so far from any individual transaction, that specialized training and experience are required to help in picking one's way through the maze. The expert is, in effect, an economist, whatever he calls himself: if he has not been trained originally he is forced to acquire the training, as he proceeds.

The field of monopolistic practices provides another example, and more generally all operations where the immediate gain to the person or agency which performs the action is different from the aggregate gains or losses to the whole community. In simple cases the considerations may be evident: anyone can see that a smoking chimney may cause some damage to one's neighbours though they do not gain from one's own fire. But most cases are more complicated. The analysis is likely to be quicker and more comprehensive when it is done by someone who has been trained to make it. And in many cases, some use of the price mechanism may be the easiest way to bring about a change if this is desired.

Examples could be multiplied—the incidence and the total effect of taxation, and the effect of wage changes, are obvious ones.

There is, finally, a prima facie case for the employment of an economist in connexion with commercial activities carried on directly by the government or its agents. I have already said that business men in general do not need economists to tell them what to do, and that this applies also to business carried on by the State. But when the State is engaged in business activities, and uses its own powers to enable these to be carried out in a

framework different from those it provides for ordinary businesses, then the criteria of efficiency and profitability which guide the business man become inadequate. This power, to make special rules for its own businesses, is used in many ways, but the most obvious ones are those in which the State either gives a monopoly to its own enterprises, or uses public revenue or public credit to support them, or both. Thus the State, if it chooses to do so, can enable its enterprise to follow courses which both the business man and the economist would consider uneconomic. It then becomes necessary to be able to weigh the divergence between the actual course followed and what the course would probably have been if the price mechanism had worked freely, so that the State can at least know what it is doing.

I do not, of course, mean that the economist should lay down the principles on which such enterprises should be conducted. It is obvious that the public, which in the last resort controls the government, does not in general think that publicly owned enterprises should necessarily be run on economic or business principles. This is clear whenever there is any change in the prices charged for goods or services produced by the government. But it is important that the arguments in favour of such principles should be set out, as well as those in favour of other principles, and it is part of the professional training of the economist that he should be qualified to see the general effects on the rest of the economic system, of pursuing special policies in any particular part of it.

To summarize what I have said so far—it is my view *first*, that although the government pursues very extensive economic activities, it does not follow that these should in general be conducted by economists: *second*, that those issues which affect the general economic climate, the level of activity, involve technical issues, and that those who have to deal with such issues need professional advice: and *third*, that where business enterprise can operate, owing to special circumstances or to the power of the State, on rules different from those applying to businesses generally, it is desirable to have a professional opinion in order to make clear the interaction between the acts of the particular enterprise concerned and the rest of the economic system. In short, the special province of the economist is the economic system as a whole, and the relation between the workings of its different parts. This is a difficult subject and requires special study.

Those who deal with these matters are in fact economists, whether they are called so or not, just as those who deal with the human body and its ills are dealing in the field of medicine, whatever they may be called. In both cases it is desirable to be trained: the life of the individual is too short to justify us in hoping for very good results from those who have to

discover for themselves, in such complex fields as these, what others have already found out.

It is also desirable, for reasons on which I shall touch shortly, that there should be a fairly intimate connexion between those who formulate and administer economic policy, and those who are advising on the technical aspects of it. Thus it is advantageous if some administrators have had an economic training, and that economists should have some experience of administration.

To pursue the analogy with medicine a little further, we should observe first that not all doctors give good advice, and second, that they themselves would admit that they have still much to learn. These propositions are much more true of the economist, and there is much more danger that he will go beyond the limits both of his knowledge and of his proper province. It seems proper, therefore, that I should say something about the kind of contribution that should be expected from the government economist and the spirit in which he should approach his work.

Within the general field I have already indicated, he has to find out the facts of the situation, analyse them, and consider the bearing of his analysis on a practical problem, that is, on what the government has to do. An academic training almost inevitably lays the main stress on the cultivation of powers of economic analysis, and in particular, on the ability to distinguish between fine shades of difference and to carry out rigid logical processes. It is perhaps for this reason that the development of mathematical methods has made so much progress among economists in recent years. The use of mathematics requires a rigid definition both of the quantities used and of the hypotheses which are involved in the assumed relations between the quantities: the results cannot be any more valid than the hypotheses or the quantities originally put in.

The government economist is dealing with the real world, and cannot select among the material available or pursue trains of thought for their intellectual interest. His first duty is to try to establish the facts and his second to proceed with great caution in formulating hypotheses about the interactions between them. This is the problem of identifying the situation, posed by the old lady who instructed her broker to sell her securities at the top of the market. The government economist who seeks to profit from the advice of his academic and journalist colleagues is often uncomfortably reminded of the difficulties of identifying a situation when he analyses the conflicting views which are showered on Ministers. He finds that the differences are hardly ever logical, and not very often about objectives, but much more often differences of view about the facts or about how the economic system will behave in response to specified action.

Although this may sound elementary and commonplace to those engaged

in the physical sciences and in the study of history in its many forms, I feel strongly that those who are dealing with current affairs, and not only economists, are very much given to generalizing on insufficient evidence. It is, indeed, extremely difficult to get the facts needed for economic analysis. Great progress has been made since 1939 in the collection of official statistics, although there are still serious gaps. In dealing with external questions, it is difficult to find out in time to be able to use the information, what is going on in other countries. But besides the facts which can be reduced to figures or plain statements, it is essential to know as much as one can about the current experience of those who are actually working the economic system, and especially how they will behave in particular situations. Economists are much more conscious than they used to be of the complexity of business behaviour, and, indeed, in this University just before the war, the Oxford Economists Research Group carried out valuable pioneer work in this field and I am glad to say is still doing so. There is still much to learn.

The government is usually well placed for the collection of information, and the government economist has access to the records and the daily contacts of the great Departments of State, which between them are in constant touch with every part of our economic life. Such departments as the Board of Trade and the Ministry of Supply are very well informed about trade and industry, at home and abroad: and the Regional Offices and, in particular, the organization of the Ministry of Labour can often give a very good picture of what is going on all over the country. In general, we know a good deal about what is actually being produced, but rather less about what is being consumed and about stocks and work in progress. We could also learn a good deal more than we do about the plans which business has made for the future.

As regards detailed situations, the partial as opposed to the total problems, the government machine in this country is very efficient and can assemble the relevant material with great skill and, if necessary, in a very short time. Since 1939, however, the great pressure of government business has put severe strains on the machine and although civil servants can turn out work of a volume and quality which astonishes those with academic or business experience, the problem of finding out even the bare facts is a serious one. An economist can often be extremely useful in assembling material of this kind.

The methods of economic analysis are now well known. For reasons which I shall be giving shortly, I do not think that very complex analytical processes are needed in most government work.

The main difficulties arise because the analysis must be of a flow rather than of a situation. This means that a good deal of attention has to be

given to the rate at which economic quantities adjust themselves to changes. For some purposes, the paths to equilibrium positions are more interesting than the positions themselves—for example, the paths involved in response to changes in the terms of trade. Flow analysis also requires us to know not only why people do things, but also the extent to which their motives will remain constant.

Thus the government economist soon becomes acutely aware of the limitations of his professional equipment. Economic behaviour is defined as behaviour tending to maximize the satisfaction which the individual can derive from the situation in which he finds himself. It is, however, extremely difficult to say what this will lead him to do at any moment, partly because we do not know what view he takes about the future and partly because we do not know enough about his preferences. I am often struck, for example, by the general sympathy felt for anyone who is adversely affected by change. When I was taught economics, we realized that the individual so affected would not like it, but we assumed that others would be indifferent, or would welcome the change if they were benefited. But I now think that the community is often willing to forgo general benefits in order to avoid inflicting particular hardships, and that this is a factor in describing actual preference systems, though one which it is hard to quantify.

I do not mean that we need to worry unduly about irrational behaviour: by and large people do choose the cheaper of two similar articles and the better paid of two similar jobs. The economist does give a fairly coherent account of economic life and of economic situations. But this is not much help in practical problems. Take, for example, the course of wages. It is of some importance that the government should be able to take a view as to how they will move. It is reasonably accurate to assume that most workers would like them to increase, and in the United Kingdom at present that increases will be asked for at regular intervals. But the factors which enter into the attitude of employers are much more complex, as are the factors which will be involved in actual negotiations. Both parties will have some desire to avoid a stoppage, and some regard for what they take to be the public interest: in addition, the existing relations in the industry, what is happening elsewhere, judgements about the state of the market, and many other factors will be involved. An economic analyst can formulate a theoretical solution, but, before this can be used, all these factors have to be quantified.

Or consider the rate of introduction of technical improvements. This is very important in making national income forecasts. It is well known that in many fields average American practice is well ahead of that of the United Kingdom. In recent years detailed studies have been made of the

difference in techniques, and many of the facts are well known in the sense that anyone can buy the reports which contain the studies. What will be the rate at which British standard practice catches up on American? How can we explain why a British manufacturer, who knows that a particular practice is more economical than his own, does not adopt it forthwith?

A number of explanations can be given, which often boil down to saying that he does not think it is worth the effort involved. But the factors which he regards as troublesome; the profit or loss differentials; and the time period with a given differential to produce the change—all these need to be known for a solution and in fact very little is known. There is still, indeed, a good deal of argument among economists as to the effect of the business climate on the rate of change. Do changes take place faster when they are necessary to avoid losses, or when demand is high so that there is plenty of money to finance changes and a good prospect of selling an increased output? Faced with questions of this kind, one generally has to fall back on a general rate of progress based on past experience.

When we attempt to look beyond the current situation, there are also considerable difficulties. The weather, political events, and technical innovations are inevitably uncertain. In the United Kingdom, where the economy is much affected by changes in overseas markets and in the terms of trade, it is not only necessary to keep a close watch on developments elsewhere, but to remember how easy it is for changes which cannot be foreseen to affect the situation.

Such uncertainties are familiar to all economists, but in practical life it is necessary to take a view about them or to make way for someone who will be braver.

Among the great range of thought among modern economists, we can distinguish two extreme positions about predictions. First there are those who hold up their hands in horror, or with satisfaction, at the complexity of human affairs and who say that the economist is inevitably so ignorant that he has nothing to contribute. I do not suppose anyone would go so far as to say that humanity as a whole should not make plans for the future on the ground that it cannot be known. Anyone who holds this view has no survival value; and equally, a government which did not take any view about the future would not be likely to survive the next election. But it is sometimes argued that an economist as such as no particular competence.

This view, so gloomy for those who have embraced economics as a profession, need not detain us long. The economic system is an articulated whole and is subject to forces which have enough regularity to make it worth studying them. Indeed, at most times it is a body with a very large momentum, which requires unusually large forces, or forces operating

over a long period, to make much change in speed or direction. It is true that there are forces at work which we cannot forecast, nor can we be sure of the effect of the forces actually operating or which may be consciously employed. But we have to live in this body and we know quite enough about it to make it rational to discuss its movements.

At the other extreme, there are some econometricians who feel that if only enough research were carried out, we should be able to make models (or sets of equations) which would cover most of the situations which actually confront us. The choice of the functions would be determined by the way particular societies behaved, and all the parameters or constants which make a general law applicable to a specific situation could be filled in from historical and current statistics. Then with the help of modern calculating machines or 'electronic brains' we could say what the outcome of a particular situation would be and could advise governments about the measures needed to produce a different outcome, if this were desired.

The government economist is trying to do this, and I am far from saying that either model making, or empirical research, is a waste of time. On the contrary, it is the only way in which progress can be made. But there is a grave danger that we may be led by the apparent precision of a mathematical expression into thinking that we know more than we do. I have already pointed out how uncertain we are about the facts of the economic world and especially about the modes of behaviour of the individuals who operate in that world. The use of any system of equations therefore involves, and must involve, a series of judgements; and in the end these will reflect a good deal of the personal experience of the economist who makes them. Those who object to the element of 'hunch' in analytical judgements are claiming far too much for the continuity and range of empirical studies. In the actual world, many of the relevant factors cannot easily be stated in quantitative terms: and it is not very likely that an equation valid at one moment will remain valid for long. The human mind, if properly trained and experienced, has a flexibility which is often able to do better than a precise and logically complete mathematical model, even with an electronic brain to interpret it.

This is a subject to which too little attention has been given. Whenever one makes an economic analysis which implies something about the future—and to be useful analysis must imply this—it is of great importance to form a clear idea of the degree of uncertainty inherent in the material. I am told that in the study of communications this is called the entropy of the system.

Suppose that it is known that an event can only occur in one or other of two ways: if a message is received through a system which is equally likely to transmit the wrong or the right signal to show how the event

happened, then no more is known about the outcome of the event after the signal than was known before. The communication system is useless for this purpose. If, however, the apparatus works more consistently, sends more right messages than it sends wrong ones, it begins to be useful. It is equally useful if it sends more wrong messages than right. As long as some of the signals are erroneous, we can never be sure of the result. But they add something to our knowledge of the situation. This something is measured quantitatively as a decrease in entropy. The entropy provides a numerical measure of the uncertainty in our expectations, and is greatest where complete ignorance causes us to treat all the possibilities as equally likely.

Most economic analysis presents similarities to an imperfect communication system. There is a particular situation, about the facts of which a good deal, but not all, is known at the moment of analysis. By study and experience much can also be learnt about the forces operating at this moment, so that some probabilities can be established about what will happen if these forces continue to act. For example, it might be ascertained as a fact that some operation is leading to profits greater by a stated figure than those being made in business generally. It may also be discovered that there are no physical or institutional obstacles to the expansion of capacity. Finally, it would be discovered that in similar situations in the past, the excess profits led to a particular rate of expansion. From these data a judgement could be made about supply and price in the future.

All the observations will have some degree of ignorance or uncertainty. We would also know that the future might unfold events which prevented the predicted outcome from taking place. Nevertheless, the result of the study can be useful—but only if we can evaluate the uncertainties will we be able to say how useful it is. We must know if the communication system sends out right and wrong signals impartially, or whether it comes out more often one way or another.

It is the intuitive realization of this state of affairs which causes practical men, when they get exactly opposed predictions from professionals, to conclude that they do not know anything about it. The system appears to them to be worthless because it gives no evidence as to how reliable it is.

Thus the economist who has to give advice ought to give the best indication that he can as to the degree to which the advice contains any genuine information. It also follows that in practical affairs it is desirable not to make decisions any earlier than is necessary if they are to operate effectively, having regard to the uncertainty involved. In most cases, though not in all, the amount of ignorance increases rapidly as one looks further ahead. It is very likely that the 8.36 will run from Oxford to London on Monday and I can undertake to attend a meeting in London

at 10.30. There is no need to promise to attend a meeting at 10.30 this time next year though it is likely that there will still be some trains, if not an 8.36. In twenty years it is very uncertain, and it is not *now* important to me or anyone else, that I should undertake any commitments, or even whether there will be trains or not.

The economist who is asked to make long-range predictions ought to confine himself very strictly to propositions, either where he thinks experience of the past justifies him, or when it is necessary to make a judgement because a decision must be taken now. Thus the growth of population can be predicted with a good deal of probability. We know that national income in most industrial countries has increased in the past and that it grows consistently with high levels of employment. From propositions of this kind we can make projections about demand on stated assumptions, which can be judged in the light of trends. It is very unlikely that such projections will turn out to be exactly right, but they can give a range which is often useful. One can also make useful hypothetical statements—for example, the statement that the national income is very unlikely to expand much faster than its supplies of basic services. It is often an object of policy to foster the growth of the national income and it is useful to know that even if this cannot be assured by following a particular policy, it can be prevented from happening unless this policy is followed.

Most public expressions about the future, such as those in economic surveys, are qualified by a number of expressions which indicate that those who use them are well aware of the uncertainties involved. And those who criticize predictions by the government, on the ground that these are generally shown to be wrong by events, are somewhat disingenuous if they omit the qualifications in the original statement. The government has a responsibility to make its own position reasonably clear, and it would be unfortunate if it became customary to subject it to technical criticism based on a misinterpretation either of the nature of prediction, or of what it actually said.

It is, indeed, much to be desired, though it is perhaps impossible, that some more precise way of expressing the degree of ignorance or uncertainty could be found. This would also be a safeguard against tendencies to state various alternatives and treat them all as equally likely. This is rather irresponsible unless one really thinks that this is the case, or that one is in complete ignorance about the probabilities.

Before leaving the question of analysis, it should be observed that it is essential to state the result of it in simple terms. A politician has got to make a decision and cannot take refuge in proving that he has not got enough information. It is no use to tell him that if each of a number of

unknowns vary, the answer will be affected in stated ways. The economist must decide himself whether he can say anything useful or not, that is, must decide the amount of ignorance in the data. If he cannot form an opinion one way or the other, then for practical purposes he must say that economic considerations need not be taken into account. This will at least clear the way to allow decisions to be taken in the light of other than economic considerations.

In general, however, one ought to be able to form an opinion and one ought not to try to cover oneself by leaving the judgement to the person who is reading or listening to advice. That is, one ought to say that the economic considerations point one way or the other, or that they are equally balanced, or that one doesn't know. And those who do not know are not much use.

In real life, however, where a decision generally has to be taken in a clear-cut way, there is often one broad economic point that is of preponderant importance and that will turn the issue irrespective of uncertainties in subsidiary points. This should be brought clearly to the foreground. Sometimes there are major uncertainties which can be explained in a way that enables them to be readily followed, and these can be put, though one's own judgement should be added. But many of the disagreements among economists are about rather small points and they conceal a great measure of agreement about fundamentals. There are hardly any propositions, considered fundamental by economists, which it is safe to assume are accepted as fundamentals in political affairs. It is much more important to bring out the dominant points than to confuse the issue with things that are quantitatively trivial whichever way they point. It is more important, in most cases, for a government economist to be able to put due emphasis on points about which nearly all economists are agreed, than to be able to find his way among economic complexities which, in practice, will often have only a confusing effect.

Although this may seem over-elaborated, I feel that it is an important matter. In the modern world the pressure of business on Ministers and on their senior officials is a killing one, sometimes literally so. They cannot be expected to weigh up complicated arguments and to form their own opinion about the balance of economic probabilities. If one goes into points that are quantitatively too small to affect the final decision, it should only be in order to show clearly that they have been considered and dismissed. But the conclusion should be clear and decisive—it should state what the main uncertainties are, and how likely in one's own opinion one or another outcome is, and where the balance lies. Just as a politician who will not make up his mind is not likely to be a good politician, so an adviser who will not give advice is not likely to be a good adviser.

Having made an analysis, the next question is that of the use to which it is to be put, that is to say, the relation of economists to policy.

It has been argued very strongly that at this stage the function of the economist comes to an end. In the field of action, at any rate by the government, there is no such thing as a purely economic action: all action is political and has to be considered in all those aspects with which government is, or should be, concerned. The economist will never be able to quantify many of the motives or desires which enter into actual decisions by human beings, whether statesmen or citizens. Yet the statesman has to weigh them all up and has to decide on what is to be done.

Although a great deal of attention has been given to welfare economics, all propositions in this field are really expressions of a moral judgement. The economist as such can only speak with authority at the previous stage, that of analysis. He can say that a particular argument is fallacious; for example, that a proposed allocation of resources adds up to more (or less) than the amount available. He can say that a proposed course of action is defended on grounds which overlook considerations which he thinks to be relevant, for example, that a proposal to change the income of one group is not taking into account consequential changes in the income of other groups. But none of these arguments are decisive. Even the purest case, in which the real income of an individual can be improved without affecting the real income of any other individual, can only be a reason for action if it is agreed that it is desirable to improve both the absolute and the relative position of the individual.

In this field one can only offer a personal opinion. The doctrine that because politicians have to take all the responsibility, the civil servant should not advise about actual decisions, is very well known and expresses a fundamental truth. It is to the government that the electorate has entrusted the power: it is the government, or the Minister on behalf of the government, who will have to answer for its actions, to abide by the result and to take the praise or the blame.

I think, nevertheless, that when a situation has been reduced to the final analysis, when all the advice including that of the economist has been put together, the right course is usually not at all obvious. If it is there is no difficulty. Either the weight of all considerations point in the same direction, or the issue can be presented in a clear-cut political form, in which it is not only the duty but the professional responsibility of the politician to reach a decision.

But if everyone can see what the answer is, almost everyone can make the decision—neither Ministers nor their advisers have anything to worry about. The difficult cases are not like this. They are the ones in which a nice judgement is needed, about the uncertainties of the future and the

balance of many considerations which do not all point in the same way. In such questions, is it fair to ask 'What would *you* do?'

I think it is. In the process of reaching a final recommendation, all those concerned, whether at an interdepartmental meeting of fairly junior officials, or at the level of final official advice, have at some stage to try to weigh all the evidence and all the considerations together, and each must try to think of how important his own particular point of view is in relation to the others. He must stand up and be counted.

Professor Robbins, in his lecture 'The Economist in the Twentieth Century', has a moving passage about the duty of economists to play their part in discussions about the ultimate ends of society. He bases this view on their training, and on the capacities of those who will settle these questions if the economist takes no part in them. But he concludes, quite rightly in my view, that if economists are to do this, they must transcend themselves as economists.

This is true also of the government economist. He ought to take part, if he is requested to do so, in discussions about decisions of policy. But in doing so, he must try to put himself in the place of those who will bear the final responsibility; that is to say, he must consider carefully not what he would do himself, but what he would do if he were in the Minister's position. This does not mean thinking only of the short run. Ministers have to think of the national interest, and to be always mindful that they are only tenants for the life of Parliament: they must think also of the present generation as life-tenants, with the duty of handing on the national estate to posterity in better shape than when they inherited it.

Thus, although a great deal of what I have said is about the economist as an expert, I conclude that he ought to be an expert in an administrative setting. If he cannot see the problems as a whole, he cannot expect that administrators will see those aspects of them which are his immediate concern. This does not mean that he must compromise his professional integrity. But it does mean that he must be conscious of its limitations.

I have tried to indicate some of these limitations. They will be forcibly brought to the notice of the economist, if in government service he is judiciously mixed with other public servants who have to deal with the subjects I have been discussing.

If we regret the comparative calm of the study, we must remember the picturesque American expression:

> If you don't like the heat, stay out of the kitchen.

SCHUMPETER'S *HISTORY OF ECONOMIC ANALYSIS*

By G. B. RICHARDSON

Introduction

SCHUMPETER'S long-awaited *History* will surely rank as one of the most important books on economics to be published in the last half-century.[1] It is very rare for economists so clearly in the front rank to write on the history of theory, and the present work is almost certainly the greatest yet written in that field. It is also a great deal more. Its author's encyclopaedic knowledge and wide vision, his psychological insight, personality, and style enabled him to make it not only a history of economics but also an immensely readable study of how thought develops in general, a study, as the author would say, of 'the ways of the human mind'. Schumpeter died before the book could be finished, but by far the greater part of it was written up in manuscripts which were put together by his wife. Thanks to her immense and complex labours, and to the assistance which she received from several scholars, the *History* now published is reasonably complete.

The great range of Schumpeter's learning as displayed in the *History* will inspire most readers with humility, and does so particularly in the case of the present reviewer, who has independent acquaintance with only a small part of the field surveyed. Each of the separate parts of the work merits a separate review article, and any discussion of the whole must be general and selective. I have attempted to deal first with Schumpeter's view of the nature of the subject and with the principles on which it seems to me his assessment of particular performances is based. I turn later to the way in which the subject develops and to the chequered life-history of some economic ideas.

2. Schumpeter on the Nature of Economics

The first part of the *History* contains a discussion of the nature of economic science which, besides being of great intrinsic interest, also serves to present the author's programme and to justify his approach. He is well aware of the futility of much methodological controversy and of the lack of correspondence frequently, and fortunately, found between our methodological professions and our actual practice. But the philosophical and methodological issues raised by the subject have a strong fascination

[1] *History of Economic Analysis*, by Joseph A. Schumpeter. (New York, Oxford University Press, 1954. $17.50.)

for his mind and he clearly felt that they could not be by-passed on this occasion, since 'the possibility of treating the history of economics like the history of any other science is controversial' and 'the very rules or principles that are to guide the historian's pen are open to doubt, and what is worse, misunderstanding' (p. 3).

The subject of the *History* is to be economics in its strictly analytic or scientific aspects. 'What distinguishes the scientific economist from other people who write and think about economic topics is', he maintains, 'the command of techniques which we class under three heads, history, statistics and theory' which 'together make up what we shall call Economic Analysis'. A 'science' is defined as 'tooled knowledge', being a field where special techniques of fact-finding and analysis are developed, and it is the story of economic thinking which is scientific in this sense that he proposes to tell. Schumpeter then identifies two pretenders who are allowed to figure only in so far as they influence Economic Analysis in the strict sense. The first of these is Systems of Political Economy which are 'expositions of a comprehensive set of economic policies that their authors advocated on the strength of certain unifying (normative) principles such as . . . economic liberalism, socialism and so on' (p. 38). These systems, apart from the economic analysis they incidentally embody, being, according to Schumpeter, 'mere formulations of the ideology of an epoch or country without validity for any other', are banished to the background of the *History*. The same sentence is pronounced for the same reasons on Economic Thought, by which is meant 'the sum total of all opinions and desires concerning economic subjects, especially concerning public policy bearing on these subjects, that at any time float in the public mind' (p. 38). With these out of the way the author now turns to his chosen field of Economic Analysis as he has defined it. Our knowledge is the product of Analytic Effort operating on raw material provided by our Vision. Only after Vision, which is 'a pre-analytic act', has isolated the set of phenomena which interest us does the analytic work of devising a suitable conceptual attire and of testing against reality properly begin. It appears that Schumpeter uses the term 'economic analysis' in two senses, as originally defined to cover 'history, statistics and theory', and in the narrower sense as a process subsequent to and excluding Vision.

Now this careful sifting, by Schumpeter, of economics as loosely defined is of course undertaken in order to separate scientific economics in as pure a form as possible from the mass of ideologically contaminated views with which it may be entangled and confused. Schumpeter accepts, with significant qualifications, Marx's doctrine of ideological bias, according to which mans' ideas and systems, while claiming objectivity, are in fact distorted by unconscious desires to justify, exalt, or further a particular nation,

class, or interest. Such systems of thought, which help us to conceive society as we would wish it to be, and which therefore correspond to rationalizations in the sphere of personal conduct, are entitled ideologies. Now once we accept the inevitability of ideological contamination of our views on economic matters, doubt is cast on the very existence of an objective and developing body of economic knowledge comparable with the established sciences. Not even Economic Analysis is free from suspicion, for ideology enters in 'on the ground floor' via the pre-analytic cognitive act referred to as Vision, which Schumpeter holds to be 'ideological almost by definition'. Our Vision 'embodies the picture of things as we see them, and whenever there is any possible motive for wishing to see them in a given rather than in another light the way in which we see things can hardly be distinguished from the way in which we wish to see them' (p. 42). On the other hand, the rules of scientific procedure followed in our analysis are 'almost as much exempt from ideological influence as vision is subject to it' and tend in the long run to purify our vision of its ideological contamination 'automatically and irrespective of the desires of the research worker' (p. 43).

This barest of outlines does scant justice to Schumpeter's arguments; but some readers who have read the original brilliant exposition may still be inclined, while agreeing in general terms, to differ on the matter of emphasis. They may feel that Schumpeter gives rather too much ground to the view that our economic opinions are invariably subject to ideological distortion and that he is therefore driven to place excessive reliance on the purifying influence of the process of technical analysis.

Should one say that 'vision is ideological almost by definition'? It may be that Schumpeter lays insufficient stress on the distinction between two different things: the existence of the more or less involuntary self-deception which we call rationalizations, and the fact that we can never think *ab initio* or view reality quite freshly, in independence of the categories and opinions which form our intellectual inheritance. The first of these represents ideological distortion in the proper sense. The unconscious motives for it may be remote and obscure. A Marxist, let us say, may convince himself, against the evidence, about the existence of certain fatal defects in the capitalist system; he may misunderstand the present reality so that it will not conflict with the principles of his faith, the unconscious reasons for accepting which—such as the desire for the security afforded by a dogmatic and all-embracing creed—may be very difficult to discover. But such distortion is not an inevitable characteristic of our vision. The second phenomenon, the fact that no one can escape from the categories in which he is trained to think, is inevitable but is not ideological delusion. It characterizes all fields of knowledge irrespective of subject-matter and is

associated with no unconscious motive except perhaps that of avoiding the unpleasantness occasioned by the disruption of established beliefs.

As our author observes, a great part of general economics is scarcely of a nature to involve us emotionally, unless we are pathologically inclined to such attachments. This applies of course particularly to work concerned with the refinement of existing conceptual schemes as opposed to the representation of reality in new ones. But even where the subject is inflammatory, can we not often hope substantially to discount our biases by consciously recognizing our fears and wishes and forbidding them to enter our scientific work? One would think that our wishes in connexion with the working of the economic system were more readily accessible to consciousness, and therefore more easily kept in order, than those, dealt with by psychologists, which cause us to have recourse to rationalizations in the conduct of our personal lives. Where ideology can and does find an easy breach is where, in considering particular cases, we have to estimate the importance of different, and almost imponderable influences, so that our guesses naturally take colour from our prejudices. But some comfort may be derived from the fact that while our judgement of particular situations may be highly susceptible in this regard, the analytical schema, or general theory which we employ, may remain much more immune.

Schumpeter, as has been said above, places very great reliance on analysis, on applying the rules of scientific procedure, as a process which tends to 'crush out ideologically conditioned error . . . automatically and irrespective of the desires of the research worker'. One might wonder whether this was not rather optimistic and underestimated the importance of the student of the subject cultivating in himself (and in his pupils) an appropriately detached attitude of mind. Intelligence is not sufficient; the detached radical intellectual who regards everyone as the victim of ideological delusion except himself is, as Schumpeter says, often 'just a bundle of prejudices which are held with all the force of sincere conviction'. Nor is the command of an apparatus of modern stainless-steel techniques a protection; it can very easily be combined with extreme susceptibility to ideological delusion. What is required is something of the self-consciousness and intellectual integrity which Schumpeter possessed in such full measure, and which was the product both of his own particular personality and of the civilization by which it was formed.

A strong conviction in the benefits to be derived from modern techniques, and in particular mathematical techniques, runs through Schumpeter's whole book. He justifies it with powerful arguments which have nothing to do with ideological contamination; but could it nevertheless be that Schumpeter welcomed these techniques also because they seemed to lift

the subject on to the securer level occupied by the physical sciences? Be this as it may, it remains curious that Schumpeter should have had such faith in mathematical methods while they feature so little in his work and hardly seem characteristic of his type of mind.

The depth of Schumpeter's concern with the problem of ideological bias is of course immensely interesting for what it reveals of the style and outlook of the man. The width of his experience of persons and places, together with his quite Protean capacity for comprehending different viewpoints as it were from the inside, must have implanted in him a deeply rooted conviction as to the relativity of human beliefs. Keynes's frequently quoted dictum that 'ideas and not vested interests are in the long run dangerous for good and evil' might have seemed to him a shallow, rationalistic dichotomy, attributing as it does to ideas an autonomy and independence of our desires which they rarely acquire. Indeed the possession by most English economists of a relatively untroubled confidence in their working philosophy and moral ideas may have been felt by him to stem from provinciality and lack of depth, though he may have envied the more comfortable assurance which it was able to afford.[1]

I wish now to turn to the general principles which seem to me to inform Schumpeter's assessment of the work of various economists. We may say that economics comprises a stock of models, a stock that is of logical patterns, of functional relationships representing schematically general cause–effect regularities in the real world. These are of all shapes and sizes, the most general isolating regularities which characterize, approximately, economic activity at almost any time or place. Others claim to represent the cause–effect relationships in a particular type of economy or over a short period of time. In any case the models have to be tailored to suit particular cases. Now economists can be classed according to the particular levels of generality at which they find it congenial to work. 'Economics', says our author, 'is a big omnibus which contains many passengers of incommensurable interests and abilities', and it is part of his greatness to be able sympathetically to understand and present all the passengers' points of view. Nevertheless we can perhaps detect the type to which he is characteristically most attracted, although the many-sidedness of his nature permits only partial validity for any such generalization. The author's personal approval seems to go out most strongly to economists of a broad and deep vision who seek to bring as great a variety

[1] Schumpeter holds that although economics 'has frequently been vitiated by the political attitudes of economists, it has not been shaped at any time by their philosophical opinions'. One wonders whether Schumpeter would still think this true in the case of contemporary Oxford. At least philosophical preconceptions influence the terms in which we wish to state our theorems, e.g. the recent behaviourist trend in the theory of consumer demand and elsewhere.

of human activity as possible under unifying general principles, and who impress us by the grandeur of their conception rather than by its immediate practical utility. They may be contrasted with writers whose work often arises from attempts to deal with current economic problems and who concern themselves not so much with the elucidation of extensive patterns of independence but with isolating just those relationships which they believe to be important for practical purposes. Marx and Walras, in their best-known work, incline towards the first group, Ricardo, Marshall, and Keynes more to the latter.[1] Schumpeter complains of the poverty of the vision of the English classical economists, who, although they lived 'at the threshold of the most spectacular economic developments ever witnessed . . . nevertheless saw nothing but cramped economies struggling with ever-decreasing success for their daily bread'. He contrasts this with the magnificence of the Marxian vision, which, 'though ideologically vitiated at the roots, hopelessly wrong in its prophecy of ever-increasing mass misery and inadequately substantiated both factually and analytically', nevertheless conceived the evolution by an inherent logic of all the interdependent activities of human society. Not everyone, however, feels the intellectual appeal of the system as strongly as does Schumpeter, for its very generality may repel as well as attract. Keynes, for example, could find nothing in it except out-of-date controversializing. Neither Adam Smith nor Marshall would probably have denied that economic change must influence some of the non-economic institutions of society,[2] but may have refrained from embodying this formally in a general theory simply because, it being extremely difficult to predict the precise form of the influence, they had no interest in formulating a relationship to which they could hope to give content. Marshall clearly states, in Appendix C to his *Principles*, that he believed a unified social science to be unattainable. What may sometimes appear as lack of imagination may, in part at least, be temperamental aversion to theorems of great generality, arising from a conviction that there is a great deal of the social process about which we have to be agnostic.

Schumpeter's predilection for theoretical generality and the glamour of great systems is suggested also by his treatment of Walras and Marshall. Walras he considered, as far as pure theory was concerned, 'the greatest of all economists', and claims that his system 'is the only work by an economist which will stand comparison with the achievements of theo-

[1] It may seem strange to bracket Marx and Walras. The similarity in their systems to which I refer is simply preoccupation with general interdependence. Marx's system illustrates interdependence of a 'vertical' kind, between the different layers of social activity, economic, political, and cultural: the interdependence in Walras is of a 'horizontal' kind, existing between all the units in economic activity.

[2] Cf. Smith's discussion of the different political structure of societies of shepherds and hunters, *Wealth of Nations*, Book V, ch. 1, part 1.

retical physics' (p. 827). Indeed there is a tendency, not necessarily unjustified, to regard the general equilibrium system as the divine event to which the whole creation moves, and to judge performances according to whether they hastened or retarded its arrival. Ricardo in particular is held responsible by Schumpeter for a 'great détour'. His patched-up labour theory of value stood in the way of advance, as did his theory of rent which 'carries meaning only within that [i.e. Ricardo's] theoretical set up and is nothing but an obstacle to the recognition of important symmetries within any other'. These charges, which are repeated emphatically by Schumpeter, may seem, at least as far as value theory is concerned, difficult fully to justify. By being based on embodied labour, Ricardo's theory of relative prices deserves to be characterized as a detour, distracting attention from the influence of demand and the whole process of evaluation of goods and productive services. Yet it may equally well be viewed as a stepping-stone, for the heavy qualifications which Ricardo places on his theory clearly point ahead. His recognition of the time element in production points in one direction, and his theory of the return to one fixed factor, that is land, can easily be generalized to cover others such as different kinds of labour. All this is to his credit, quite apart from the fact that he brought into economics a new spirit of analytical sharpness and precision.

Marshall is likewise charged by Schumpeter of obscuring the pattern of mutual interdependence. This he did in the famous scissors analogy, by claiming that the demand and utility analysis of Jevons and others, and the cost analysis of Ricardo and the classics, were each only half the story. In fact, Schumpeter argues, a proper general equilibrium analysis shows that both blades of the scissors 'consist of the same material. . . . both demand and supply . . . can be explained in terms of utility'. It is difficult not to agree with Schumpeter that Marshall was generous to Ricardo at the cost of being less than just to Jevons; more important is it to notice that from the point of view of partial analysis (whether or not we believe the usefulness of this simplification to outweigh its dangers) Marshall's approach is quite appropriate.

Now, the conception of general interdependence was, as Schumpeter points out, within Marshall's range and is actually set down in embryonic form in Note XXI of the Mathematical Appendix to the *Principles*. It was not lack of imagination, and certainly not lack of mathematical ability, which prevented Marshall from elaborating it; but rather, it would seem, instinctive moral scruple at formulating theories which he felt to be of aesthetic rather than utilitarian interest. He seemed to have wished to select only those parts of the whole skeleton of economic relationship which he could clothe with flesh and blood and to keep all the rest in the

background. Schumpeter quotes his highly revealing remark that his 'whole life has been given and will be given to presenting in realistic form as much as I can of my note XII'. Marshall's characteristic approach derives no doubt from his particular temperament and from the nature of the motives which led him to devote his great talents to the service of the science. The driving force in Schumpeter seems to have been the spirit of pure intellectual inquiry, which in Marshall was secondary to the desire to ameliorate human life through the relief of poverty. Thus we see again that there is no simple criterion for assessing performance even in theoretical analysis; for concepts may be appraised on their purely intellectual appeal, which usually derives from the way in which they co-ordinate under a few unifying principles a wide range of ostensibly separate phenomena, or to the extent that they form 'short links' (to use Marshall's term) of great use in the study of particular economic situations.

So strong was Marshall's desire to hold his discussion down to real economic life, his 'bent', as Schumpeter calls it, 'towards misplaced realism', that the opaque façade of his work partly conceals the careful analytical scaffolding he has erected behind it. The danger in such an approach is that it may gain the very doubtful short-run advantage of applicability only at the cost of retarding the progress of the science. But the reluctance to expose theoretical schema in clear and naked outline may have arisen from Marshall's fear that they might harden in the minds of others into a more reliable representation of reality than he himself, with his sense, as Schumpeter calls it, of the 'intimate organic necessities of economic life', ever believed them to be.

Though Schumpeter appreciated Marshall's greatness as an economist and pays generous tribute to his insight into the facts of economic life—which made him speak 'as one who has power and not like the scribes'—yet one clearly notices how different the personalities and philosophies of the two men were. To Schumpeter, Marshall's manner, his tendency to patronize and his 'propensity to preach', and his lack of generosity in acknowledgement, are clearly uncongenial; his conception of the Noble Life ludicrously naïve and insular, and his utilitarian approach an obstacle, at least on occasions, to the progress of the science.

Schumpeter's appraisal of Keynes has already been recorded, before the publication of the *History*, in the magnificent memorial article[1] which recalls, in the skill with which understanding the man is related to appreciation of his work, Keynes's famous essay on Marshall. And the appraisal is particularly revealing both of our author's approach and of the nature of our subject. There were similarities between the personalities of Schumpeter and Keynes, but in approach to the subject they differed greatly.

[1] *American Economic Review*, 1946.

Keynes's kinsman in aims and method was, according to Schumpeter, David Ricardo. He says of the latter:

> In all the questions he touched he was on the side that would have won out anyhow, but to the victory of which he contributed a usable argument, earning corresponding applause. Though others did the same, his advocacy was more brilliant, more arresting than was theirs; there is no superfluous sentence in his pages; no qualification *however necessary* weakens his argument; and there is just enough genuine analysis about it to convince practically and, at the same time, *to satisfy high intellectual standards* but not enough to deter. His polemical talent, which combined to an altogether unusual degree readiness, force and genuine politeness, did the rest. People took to his theory because they agreed with his recommendations . . . (p. 473). [Author's italics.]

He then adds, in a footnote, that every word of this applies to Keynes. Ricardo's method of work is also found to resemble that of Keynes, and he says of the former:

> His interest was in the clear-cut result of direct practical significance. In order to get this he cut that general (economic) system to pieces, bundled up as large parts of it as possible, and put them into cold storage—so that as many things as possible should be frozen and given. He then piled one simplifying assumption upon another until, having really settled everything by these assumptions, he was left with only a few aggregate variables between which, given these assumptions, he set up simple one way relations so that, in the end, the desired results emerged almost as tautologies (p. 472).

Keynes is also found guilty of this 'Ricardian Vice' by which Schumpeter means 'the habit of piling a heavy load of practical conclusions upon a tenuous groundwork, which was unequal to it yet seemed in its simplicity not only attractive but also convincing' (p. 1171).

Keynes's work is offered by Schumpeter as a striking example of the distinction between analysis and the vision which precedes it, the vision in this case being that of England's ageing capitalism, 'the arteriosclerotic economy whose opportunities for rejuvenating venture decline while the old habits of saving formed in times of plentiful opportunity persist'. In writing the *General Theory*, he argues, Keynes 'bent to the task of framing an analytical system that would express his fundamental idea *and nothing else*' [author's italics].

These criticisms point very clearly to the different approaches to economics described above. Keynes, like Marshall, though of course susceptible to the intellectual appeal of the subject, regarded economics primarily as a useful art designed to guide policy. Given this outlook, the model he sought to construct had to be usable and had therefore to bear close reference to the particular reality with which he was concerned and had to be reasonably simple. The 'General Theory' is therefore a tailor-made model, though it is surely of much wider applicability than Schumpeter would allow it. It can, for example, as everyone knows, be used in the

analysis of inflation as well as of slumps. It does not of course represent the working of any conceivable capitalist economy; a really general theory of this kind would probably be of little practical use.

Keynes's procedure of making severe assumptions so as to reach clear-cut conclusions is of course risky, and one relies partly on his judgement and intuition that the conclusions would still approximately hold even if the conditions did not quite strictly obtain. But before reaching a policy decision judgement or guess-work inevitably come in at more than one stage, both in framing the model and in the application of it. All we must demand is that the assumptions or guesses be fairly explicit and if possible capable of verification, a condition which the 'General Theory' reasonably satisfies. Finally there is the ultimate test, also met, of success, not in the sense of gaining adherents, but in having in fact actually assisted policy-makers in choosing the right means for the end they desire.

3. The development of the subject

We have noted that Schumpeter's conception of the *History* is based on a distinct view of the nature of economic science: further testimony to the systematic character of his mind is that the division of the book into historical periods is likewise according to a clear principle. Advance in the subject is seen as neither steady nor uniform, but as involving many detours and the loss of positions gained earlier. A certain pattern is, however, to be observed, in which long periods of confusion and controversy are followed by a time of systematic consolidation, characterized by general agreement between economists on fundamentals and self-satisfied confidence in the validity and permanence of the positions reached. Such periods of repose are referred to as Classical Situations, and Parts II, III, and IV of the book deal with the three great periods which culminate in this way. Part V presents a sketch of modern developments, which though of great interest, is not comparable with the previous parts. The first and longest part is 'from the beginning', up to the second half of the eighteenth century. It covers principally the ancient world, the medieval schoolmen, the 'more boisterous stream' of Consultant Administrators and Pamphleteers, the Physiocrats with Turgot, and finally, as the great figure of the Classical Situation with which the period closes, Adam Smith. The second period ends with the 1860's and is 'summed up in the typically classical situation . . . of John Stuart Mill, who underlined the fact by his attitude of speaking from the vantage point of established truth and by the naïve confidence which he placed in the durability of this established truth'. The third period is rounded off by the leading works of the last decade of the nineteenth century which again created 'in the superficial observer, an impression of finality'.

This development exhibits, to a much greater extent than in the sciences of matter, waste and inefficiency. First, there is no orderly inheritance of ideas; the gains of one generation are frequently jettisoned by the next according to the swing of the doctrinal pendulum. Secondly, many ideas find their way only belatedly into the corpus of knowledge, having been entirely ignored when first expounded.

The history of interest theory offers a splendid, or rather melancholy, example of how established positions can be lost. It begins effectively with the remarkable contribution[1] of the late scholastics, particularly Molina, who conceived of interest as depending on the demand and supply of money and went a long way to explaining the factors which determine its rate. The demand for money was seen to arise directly from the fact that it was the 'Merchant's Tool' and therefore indirectly from the fact of business profit. Payment for the supply of money was justified not only on account of the risk and trouble of lending but as a compensation for any losses which forgoing command over money might involve. This view of interest would appear to have persisted in the seventeenth and well into the eighteenth centuries when it was expelled by a simpler doctrine enunciated probably first by Barbon and supported by Turgot and Smith. This concentrated solely on changes in saving and productivity as affecting the demand and supply of loans. Indeed it went further; it insinuated that interest was *directly* dependent on productivity and thrift so that money could be left out of the analysis. This tended to result from the language employed. 'Interest is commonly reckoned for Money', said Barbon, '. . . but this is a mistake; for the interest is paid for Stock'. 'It is the Rent of Stock and is the same as the Rent of Land.' Thereafter the complexity of the factors affecting interest through the demand and supply of money (which had thus been effectively short-circuited) were ignored except by a few writers (one of them, Thornton, of great distinction) until relatively recent times.

Further light is thrown on the wayward nature of the development of our science by the fate of the foreign-trade theories of the mercantilists. To Smith and the economists of the classical period these theories and 'that imaginary organon, the mercantilist system', were the object of indignation and derision, as simply error, and pernicious error at that. Later on an equally excessive reaction developed, principally among German nineteenth-century economists with protectionist leanings. In attempting to strike a proper balance Schumpeter himself distinguishes between two questions: whether mercantilist policies offered adequate means towards rationally defensible ends, and whether the theoretical

[1] This was not the only remarkable contribution of the schoolmen, as Schumpeter, in one of the most interesting parts of the *History*, clearly shows.

arguments used to justify them were valid. On the first count the mercantilists acquit themselves quite well, given the fact that they lived in underemployed and underdeveloped economies and were deeply concerned with international power politics. But the arguments offered were mainly pre-analytic and popular 'in the most distressing sense of that word'. They did, however, have important insight and the protectionist arguments seemed to be remarkably up to date—infant and key industries, home employment, military needs, even the terms-of-trade argument all put in an appearance.

Now what we have to note here is that both the contempt and the favour with which the mercantilists were alternately viewed was in fact stimulated, not by their contributions to the science, but by their policy opinions, according to whether these appeared in harmony with the prevailing ideology. So strong were emotional attitudes towards 'monopoly' or 'free trade', that they denied to later critics the detachment required to separate the scientific contributions from the policy recommendations. Adam Smith in particular was content to ridicule a mere caricature of mercantilist doctrine and, thereby, because of his immense authority, to do disservice to the science.[1] The situation as Schumpeter puts it was 'comparable to the waste which would result if successive teams of workmen smashed the products of their predecessors whenever they disliked the latter's politics'. It certainly goes a long way to justify the stress which he places on the importance of ideological influence on economic thinking. It also suggests an important reason why, at any particular time, at least a few economists should be studying the history of the subject in an attempt to regain lost ideas.

So far we have dealt with the abandonment of economic theories because of the policy views or ideologies with which they were associated, and notwithstanding the real insights which they may have contained. Economic theory may also be retarded simply because new ideas failed to be noticed or did not catch on. The marginal utility theory of value, for example, for which credit usually goes to Jevons, Menger, and Walras, was in fact anticipated not only by Gossen and Dupuit but also by a Student of Christ Church and Drummond Professor of Political Economy, at Oxford, W. F. Lloyd, whose demonstration was, according to Schumpeter, 'quite straightforward' and had 'nothing deterrent about it'.

The causes of this phenomenon are probably several. In the first place, as Schumpeter observes, 'analytical progress—not only in economics—hinges in great part on making things explicit which have been implied

[1] This is not to say that Smith's condemnation did not have a beneficial influence on the policies of governments and on world affairs, and that the influence would have been less strong if the condemnation had been more qualified.

or implicitly recognised for ages'. We are capable, that is, of 'knowing' an economic fact on two levels, on an everyday, unanalytic level, or systematically, in such a way that the relations of this fact to the rest of our economic knowledge is observed. An example of this is the unwillingness and difficulty with which professional economists right into the twentieth century admitted into their general schemes of thought the fact that banks create credit. Secondly, until the nineteenth century the study of economics was comparatively unprofessional, so that it was possible for the views of individual thinkers to fail to gain a substantial audience and thereby to be accepted into the corpus of doctrine. But this is far from the whole story, as the previous example of W. F. Lloyd clearly shows. A deeper reason may be that innovation in economics consists not so much in the discovery of new facts but in conceiving a new way in which these facts can be viewed. Theories are indispensable for any understanding of economic phenomena, but once entrenched in our minds they bitterly resist any successors by which our understanding may be improved. 'New ideas', as our author observed, 'unless carefully elaborated, painstakingly defended and pushed, simply will not tell.'

The conditions for popular success are discussed by Schumpeter with reference to the most famous of all books on economics, the *Wealth of Nations*. He regards it as the conclusion of the first of the three great periods. It is seen not as the foundation of the science but as a work of consolidation, the fact being, Schumpeter claims, that 'the *Wealth of Nations* does not contain a single analytical idea, principle or method that was entirely new in 1776'. Originality is, however, surely difficult to define in this field, where ideas rarely tend to rise fully armed but tend to grow, by increasing clarification, from obscure origins; and, as Schumpeter himself observes, if eminence in an economist be judged solely in terms of entirely new results not only Smith, but Ricardo, Mill, and others might fail to justify their reputations. Schumpeter accounts for the spectacular and enduring success of the *Wealth* partly from the fact that it marshalled things the way they were going; it offered cogent and eloquent arguments in favour of policies which, as Schumpeter points out, already commanded increasing popular support. Moreover although, to quote Schumpeter, it 'entirely lacked the graces of the *Esprit des Lois*', yet it was immensely readable, free from recondite truth or difficult methods, never moving 'above the heads of even the dullest readers' but leading them on 'gently, encouraging them by trivialities and homely observations'.

Although Smith may bear a large part of the blame for the impoverishment of the theory of interest and, perhaps, of international trade, yet in other directions the *Wealth of Nations* would not seem to have too

narrowly conditioned future development. For the consolidation effected by Smith was of a loose and unsystematic kind, and those who have studied the *Wealth* to discover his views of particular subjects will know how difficult it is to extract any single consistent doctrine. But this very defeat made the book a fertile source of future work, both Ricardo and Malthus, as Schumpeter says, representing different ways of recoining the *Wealth*. Smith suffers a probably justifiable reduction in status at Schumpeter's hand; his mind was not remarkably original and certainly lacked the cutting edge of, say, Turgot or Ricardo. Even his confusions, however, are, I suspect, worthy of study, for they may sometimes embody an obscure insight not given to clearer-minded economists. Schumpeter's attitude to Smith is fair if unsympathetic. He clearly finds uncongenial the personality of the painstaking, circumspect, professorial Scot, to whom 'the glamours and passions of the world were just literature', 'everywhere turning his Chair into a seat of judgement and bestowing praise and blame' on the basis of his 'dry and uninspired wisdom'.

The different rates of progress which economics has made at different times and places leads our author to some interesting reflections on the conditions most favourable to analytic advance. The pressure of great current economic problems often provides the stimulus for development, but it is far from being a sufficient condition. The great contributions arising from the controversies over the Restriction Act of 1797 and subsequent monetary crises were the work of men of affairs primarily interested in practical measures, but of men who also possessed the 'taste and ability for theoretical analysis' without which experience by itself fails to promote scientific advance. Ancient Romans had economic problems in plenty and had great ability; but what remains of the writings suggest that they lacked the necessary scientific curiosity and bent to theoretical speculation. The absence of important original work in the United States during the second of Schumpeter's great periods is attributed to similar causes. Practical problems once more were there and also the demand for economic teaching, but the tasks and opportunities presented by the business environment absorbed the energies of the best men, so that 'the brains that could have done the job were producing boots'.

Schumpeter refers more than once to the great difficulty which the human mind has in forging the most elementary conceptual schemes. One is led to reflect that in economics, as elsewhere, any original improvement in our knowledge of reality is only made possible by withdrawing from it. Under the immediate pressure of reality, in its particular or everyday aspects, we cannot hope to escape from the tyranny of the conventional categories through which we view it. To see problems in the obvious and to find the familiar strange is an indispensable, if unnatural, step to take.

4. Conclusion

The *History* is surely one of the most readable books ever written on economics, a quality which it owes above all to the remarkable personality of the author, which enlivens every page. One is obviously tempted to make comparisons with Keynes. Clear brilliance, authority, quickness and flexibility of mind, and a sense of fun seem common to both. Both seemed in reaction against the bourgeois virtues and had a strong personal aversion to utilitarianism as, to quote Schumpeter, 'unsurpassably shallow as a philosophy of life'. But in other ways they differed fundamentally. Schumpeter was, I should imagine, Keynes's superior as a scholar, at least within the field of economics, where the latter's knowledge of the literature, to quote our author, was 'not of the first order'. Not that Schumpeter is academic in the derogatory sense; he himself rather despises the intellectuals 'who know business only from the newspapers'. To some, Schumpeter's vision of life will seem less space- and time-bound, deeper if more pessimistic than that of Keynes, his work more systematic and thorough. Largely it is a difference in nationality; Keynes was surely exceedingly English and in direct descent from the English empirical philosophers. At least in one sense he was more successful than Schumpeter in that he probably made a greater impact on economic thought; though Schumpeter refers with scepticism and wry humour to the former's belief 'that he had led economics out of 150 years of error into the land of definite truth'.

No simple account of Schumpeter could be true. His thought displays both strongly metaphysical and strongly empirical strands, and has both Germanic and Anglo-Saxon affinities. The whole book is charged with his electric personality, yet scarcely any mention is made of his own contribution to the subject. His English style is both dignified and familiar, his vocabulary both scholarly and colloquial. He displays, in fact, all the bewildering contradictions of genius.

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THE FOUNDATIONS OF REVEALED PREFERENCE THEORY[1]

By PETER NEWMAN

I. Introduction

1. THERE is an increasing tendency in modern economic theory towards greater rigour both in the formulation of its assumptions and in the standards of proof which those assumptions imply. This tendency, far from being 'too abstract', is to be commended on several grounds, one of which is that it *is* rigorous, and we can therefore have greater confidence that our conclusions really do follow from our hypotheses. An equally important reason for welcoming this approach is that we are often able to dispense with unnecessarily unrealistic economic assumptions, such as perfect competition and divisibility, and unnecessarily restrictive mathematical conditions, such as differentiability, in the proof of the desired results. Thus this method of attack leads at once both to more precise theory and to greater realism.

To say this is certainly not to favour this method to the exclusion of all others: nor is it to endorse its indiscriminate application, for a devotion to rigour for its own sake can lead to mere formalism, even in mathematics itself. Economic content is, as always, the chief criterion by which to judge the results reached by this method.

Its main disadvantage is that the level of abstraction on which it operates is at present often difficult for economists to understand. This unfamiliarity may be expected to lessen as time goes on, and in any case does not constitute a valid objection against its employment.

This paper, as a small example of this method, presents the theory of revealed preference in reasonably rigorous terms. As a result of a careful formulation of the usual assumptions, a number of unresolved problems in this field can easily be cleared up. In particular, the vexed problem of integrability can be assigned its proper, minor, place.

2. First a few properties of the usual ordinal preference theory may be recalled. This theory assumes that the consumer can compare all possible

[1] Part of this paper was written at Stanford University, with the support of the Office of Naval Research. My thanks are due to Patrick Suppes for several helpful discussions, to Kenneth Arrow and H. S. Houthakker for their comments on a previous draft, and to Professor J. R. Hicks for kindly allowing me to read the relevant chapters of his forthcoming book on the theory of demand. I must, of course, take all responsibility for any errors remaining. The references cited will be found at the end of the paper.

commodity bundles with an arbitrary bundle x, in such a manner that

(a) x is preferred to some bundles,

(b) some bundles are preferred to x, and

(c) some bundles are indifferent to x.

The consumer's preferences are then represented by some utility function U, unique only up to a strictly monotonic transformation, with the property that the utility attached to a bundle x is greater than the utility attached to a bundle y if and only if x is preferred to y. It is then assumed that the consumer endeavours to maximize his utility function, subject to the constraint of a limited income. This is a simple constrained maximum problem, to be handled by the usual Lagrangian techniques, and the main complication is that we are only interested in theorems that are invariant under a strictly monotonic transformation of the utility function.

3. All this is, of course, well-trodden ground, and the only purpose in recapitulating it here is to emphasize that it is the initial assumption of the completeness of the ordering of the consumer's preferences that is the important one. The representation of these ordered preferences by a utility function is, in an essential sense, merely an addition, a convenient way of representing this preference ordering [cf. Arrow (1), pp. 11–17; Debreu (1), pp. 276–7]. It is the confusion between these two concepts, the fundamental one of the completeness of the preference ordering, and the secondary one of the utility function, that accounts for the bulk of the controversy over the integrability problem.

4. The conditions for a complete weak ordering are very simple[1] [cf. Arrow (1), pp. 13–14; Tarski (1), pp. 90 ff.]. Let the set of all possible commodity bundles be X, and denote the relation 'preferred or indifferent to' by R. Then the necessary postulates are

AXIOM I. *For all* x^1, x^2 *in* X, *either* $x^1 R x^2$ *or* $x^2 R x^1$;

AXIOM II. *For all* x^1, x^2, x^3 *in* X *such that*

$$x^1 R x^2 \quad \textit{and} \quad x^2 R x^3,$$

we have $x^1 R x^3$.

The first axiom stipulates that all points in the 'commodity space' X are ordered by the relation R; the second asserts the transitivity of this relation.

Providing that the ordering of X induced by R satisfies certain other very mild topological conditions [cf. Debreu (2)], which is usually the case, it may be represented by a utility function with the desired properties.

[1] A 'weak' ordering, since equality with respect to R may be admitted. A 'strong' ordering relation is one in which no elements can be equal with respect to the relation, e.g. the relation 'greater than' in the field of the real numbers.

5. Now it so happens that the slope of an indifference curve at a particular point x may be represented by a simple type of differential equation. Only under certain restrictive mathematical conditions, however, may this differential equation be integrated to give its solution as a unique 'indifference' curve through x. This is the integrability problem. For many reasons it seems a problem foreign to economists, for the integrability problems *as such* have no clear economic meaning; and yet they seem to be essential for the resolution of an important economic problem. One's economic intuition seems to sense that something is wrong, and from our present position it is easy to see what it is. Suppose that the integrability conditions were satisfied, so that a unique curve satisfying the differential equation passed through x. If this solution curve really is an indifference curve, then it must be proved that all the points lying along it are indifferent to x. In order to do this it can be shown that it is necessary—and sufficient—to adopt Axioms I and II. If these *are* adopted, however, then we have assumed the conditions for a complete ordering, from which a utility function, and hence indifference curves, can be obtained directly.

6. These results were brilliantly proved in 1936 by Georgescu-Roegen (1), where he showed that, even in the case of two goods, when the differential equation defining an 'indifference element' is always integrable, it is necessary to assume that indifference is a transitive relation in order to show that the integral curves *are* indifference curves, i.e. that all points lying along any such curve are indifferent to each other. Georgescu-Roegen then showed that the transitivity condition of itself guarantees that the differential equations are integrable in every dimension; so that the integrability conditions as such are redundant.

Thus the problem of integrability is an essentially minor one. Unless Axioms I and II are adopted, the integral curves have no economic meaning. If those axioms are adopted, then the integrability conditions are automatically satisfied.

II. Some current approaches

7. In this section I shall discuss briefly a number of papers to be found in the literature which have some connexion with the theory of revealed preference, due to Samuelson [(1)–(6)] and Houthakker (1), that forms the main topic of this article.

8. First we shall take the work of Herman Wold [(1)–(3)] in this area of theory. In his first paper, published in 1943–4, he analysed the 'demand-function' approach adopted by Cournot and advocated by Cassel.[1] Using

[1] Wold used similar techniques in the same paper to show that the 'marginal substitution' approach, due to Allen, is logically equivalent to that of ordinal preference theory; his proof for this case also is vitiated by the considerations discussed below. See Wold (1), pt. i, pp. 110–11.

techniques remarkably similar to those of Samuelson, though, of course, independently, Wold attempted to show that the assumptions of this approach lead to a self-contradiction unless the integrability conditions (or conditions equivalent to them) are assumed. If these conditions are assumed, however, then Wold claimed that they imply the existence of a utility function. Thus the demand-function approach is logically equivalent to the ordinal preference approach. Recently, Wold [(2), (3)] has asserted that this method of proof goes through for Samuelson's revealed preference theory, and that Houthakker's theorem [Houthakker (1)] is essentially the same as his.

9. However, a careful reading of Wold's proof [(1), pt. iii, pp. 80–88; (3), pp. 92–93] shows that he implicitly assumes that *his* 'revealed preference' is a transitive relation, e.g. in (1), pt. iii, p. 82, Wold states '. . . a' is preferred to any budget [bundle] on or below L'. Observing that a is preferred to a', it follows that a lies above L'. *Accordingly, a is preferred to every budget in that region . . . which lies above L but below L'.*' (Italics mine.) Using this assumption and the usual assumptions of perfect competition, he can show that, for a given commodity bundle x, the boundary between the points that are preferred to x and those that are disfavoured to x is a convex curve in the commodity space through x. It is not possible, however, to say anything concerning points on this boundary curve, and it is a *non sequitur* to identify such a boundary curve as an indifference curve, as does Wold. In order to do this it must be shown, as we have seen, that all points lying along it are indifferent to x. Since Wold does not define indifference, this is impossible to do.

10. Since it is not possible to assert that the boundary curve discovered by Wold is an indifference curve, it cannot be said that he has succeeded in deducing the existence of a utility function from his assumptions. It should be remarked that his integrability assumptions are redundant, since the existence of a convex boundary curve flows from his implicit assumption of the transitivity of preferences, and not from his explicit assumptions of integrability.

11. The paper by Ville (1) should be mentioned briefly. While his treatment is superficially similar to that of Samuelson, it is not in fact comparable. Starting from certain restrictions on the consumer's market behaviour, he deduces the existence of a utility function by means of a very elegant analytic proof. It can, in fact, easily be shown that a reasonable interpretation of his axioms, which are not entirely clear, leads to a relation that induces a complete weak ordering on the commodity space. Although the proof is not at fault, the axioms he postulates are open to criticism. In

particular, his criterion for the consumer being worse off is inadequate,[1] as may be seen by applying Samuelson's more reasonable 'weak axiom'. Ville's criteria are that

(i) $\sum_i p_i dq_i > 0$ (where p_i are the prices in the first situation, dq_i the change in quantities in going from the first to the second situation) indicates that the consumer is 'better off' in the second situation than the first;

(ii) $\sum_i p_i dq_i < 0$ indicates that the consumer is 'worse off'.

Criterion (i) is the same as Samuelson's criterion for being 'better off': but few economists would accept (ii) as being a valid criterion of being 'worse off'.

12. It should be emphasized here that while the integrability problem crops up as soon as the commodity space contains at least three goods, the transitivity problem arises as soon as three *situations* are considered. Thus it is bound to arise even in the two-good case. The treatments by Little [(1), (2)] explicitly assume that revealed preference is a transitive relation, but do not show that the points lying along the boundary curve are equivalent to each other; so that his analysis also does not yield a utility function.

III. The Nature of Revealed Preference

13. With these results in mind, we now turn to the theory of revealed preference, which is due mainly to Samuelson [(1)–(6)]. It is important first of all to discuss the aims of this alternative approach to ordinal preference theory, for by being careful to define what it is that the theory is trying to do we will be less liable to fall into the subtle traps with which this field is so liberally set.

14. Obviously the main reason for adopting the approach is a strictly econometric one. The investigator is assumed to have a number of observations on prices and quantities relating to a single consumer,[2] and the consumer's behaviour is assumed to obey certain consistency conditions. From these consistency conditions we wish to derive many theorems in demand analysis, e.g. that quantities demanded will remain unchanged when all prices and income are increased in the same proportion.

[1] This was pointed out to me by Mr. Robert Clower.

[2] The theory must refer to a single consumer, for market observations involve the interpersonal aggregation problem. Even with such narrow units as households there are essentially similar difficulties, cf. Arrow (1), ch. ii, and Samuelson (5). It is interesting to observe that inequalities relating to market behaviour, exactly analogous to those given by Samuelson for individual behaviour, were deduced by Wald (1) as one of the set of conditions sufficient for the existence of a solution to the Walrasian general equilibrium system of equations.

The problem then arises of just what consistency conditions should be assumed, for they clearly should not be entirely arbitrary. Considerations of analytical convenience of course play some role in their selection, but the main reason for the adoption of the first consistency conditions proposed by Samuelson [cf. (3), chap. v] was that they were suggested by a consideration of the usual theory of ordinal preference. A consumer acting in accord with the principles of this latter theory would obey the consistency conditions proposed by Samuelson.

It does not follow, however, that because these consistency conditions were derived from ordinal preference theory they should themselves imply that the consumer actually has an ordered system of preferences. The consistency conditions that we adopt may only be necessary conditions for the existence of a completely ordered preference field, not sufficient conditions.

15. This brings into relief the purpose of revealed preference theory. Two objects of the theory may be distinguished here, though there are probably others:

(i) We may wish to find out just what conditions on the set of observations on prices and quantities are necessary and sufficient to ensure that the consumer has a complete preference ordering; or

(ii) We may want to test the hypothesis that the consumer has a complete preference ordering.

16. If we are discussing (ii), it is clear that we will never be able to establish unambiguously the existence of a preference ordering—defined over the whole commodity space—from our observations, for such an ordering would require us to test the comparative ranking of every point in the commodity space. In other words, a non-countable infinity of points would have to be tested.[1] Therefore all that we can do is to inquire whether the observations contradict the hypothesis that the preference ordering exists; if they do not, then there exists a presumption, however slight, that a preference ordering does exist.

The preceding remarks are, of course, just an obvious application of the fundamental principles of the theory of testing hypotheses, but they have an important implication in this context, for if we are concerned only to contradict the hypothesis, then we need only deal with consistency conditions that are *necessary, or necessary and sufficient* for the existence of a complete preference ordering; we need not concern ourselves with sufficient conditions.

[1] It might be desirable for some problems to consider a utility function defined over the sample space of observations, in which case the considerations in the text do not apply.

17. It is, however, with problem (i) that we shall be concerned in this paper. The main problem is to establish directly a complete ordering of the commodity space from the assumptions of revealed preference. Once this ordering has been obtained, a utility function can always be chosen to represent it, subject to the conditions discovered by Debreu (2).

18. At this point it will be worth while to recapitulate the essential features of the theory of revealed preference. A consumer is assumed to have a given income, say M^0, and to be faced with a given set of prices, say p_i^0. Then it is said that the commodity bundle x^0 $(= x_1^0, x_2^0, \ldots, x_i^0, \ldots, x_n^0)$ that he selects from the collection of commodity bundles that can be bought with that income and at those prices is *revealed preferred to* the rest of the commodity bundles in that collection. This is the definition of the relation 'revealed preferred to'. In index number terms, if x^0 is the batch selected, and x^j any other batch in the collection available

$$p^0 . x^0 \geqslant p^0 . x^j,$$

where we use the notation $p^0 . x^0$ to mean the sum of cross products

$$\sum_{i=1}^{i=n} p_i^0 x_i^0.$$

19. At this point an ambiguity needs clearing up. We have used the terminology 'revealed preferred to', but it is clear that this is too narrow a term. We cannot plausibly argue that just because x^0 was bought rather than any other batch in the available collection, therefore x^0 is better than any other such batch. For there may be a batch, say x^k, which is actually indifferent to x^0, but which was not in fact bought. If the same situation occurred again, x^k might be bought instead of x^0.[1]

Therefore the relation that we have defined should really be described as 'revealed preferred or indifferent to'. If we are not prepared to broaden the concept in this way, then it is intuitively obvious that we shall never be able to establish a weak ordering, for we shall not admit equality with respect to the relation; and therefore we shall not be able to establish a complete system of preferences.[2]

It is therefore up to us to decide which path to take, whether to broaden the wording of the relation or not. I have chosen to assume for the time being the 'broad' interpretation, since it seems much the most intuitively

[1] In order to test, on the basis of revealed preference theory, whether two situations are indifferent, we should have to arrange for a sequence of experiments with the same consumer under exactly the same conditions, and employ usual statistical methods to test the hypothesis that two (or more) points are indifferent. We can never be sure that two bundles are indifferent, but we can place definite bounds on our confidence that they are in fact so. Cf. the discussion between Kennedy (1) and Little (3).

[2] This position is really not valid, as we shall see below in connexion with Houthakker's analysis.

appealing approach. It should be noted, however, that Samuelson seems often to lean to the former 'narrow' interpretation.[1]

20. Let us denote the relation 'revealed preferred or indifferent to' by R. This corresponds to the condition, if $x^0 R x^1$,

$$p^0(x^1 - x^0) \leqslant 0.$$

It is clear that we have either $x^0 R x^1$ or not $x^0 R x^1$, since R is just a comparison of value sums, i.e. of real numbers.

21. Samuelson was led to define his first consistency condition, the 'weak axiom', by the intuitively appealing consideration that if x^0 and x^1 are two distinct points and x^0 is revealed preferred to x^1, then x^1 should not be revealed preferred to x^0. His axiom, which we shall call Axiom 2*a*, then reads:

If $p^0(x^1 - x^0) \leqslant 0$, *then not* $p^1(x^0 - x^1) \leqslant 0$,

i.e. if $p^0(x^1 - x^0) \leqslant 0$, *then* $p^1(x^1 - x^0) < 0$.

From our point of view this axiom of Samuelson's may be considered too restrictive, for it is clear that we may have that x^1 is revealed preferred or indifferent to x^0 if, in fact, the two points are indifferent. For the moment we shall not rule out this possibility.

22. Nevertheless there is a strong case to be made out for the condition that if x^0 is not revealed preferred or indifferent to x^1, then x^1 must not be revealed strictly preferred to x^0. Unfortunately we have not yet defined what we mean by 'revealed strictly preferred to', and this we must now do. In order to do so, however, we must make explicit an assumption which is usually not stated in revealed preference theory, and which is essential for its validity, at least in the forms in which it is usually presented. We need first a few elements of notation.

By $x^1 \geqq x^0$ we shall mean that the commodity bundle x^1 is greater than or equal to x^0 in every component (good) contained in the bundle. By $x^1 \geqslant x^0$ we shall mean that x^1 is greater than or equal to x^0 in every component, and greater than x^0 in *at least one* component.

We now frame the postulate of non-saturation, Axiom 1. If two commodity bundles x^0, x^1 in the collection available to the consumer with a given income and given prices are such that $x^0 \geqslant x^1$, then the consumer will select x^0.

23. This postulate implies that the only effective set of commodity bundles from which the consumer will choose lies on the price line, for any

[1] If one makes the usual assumptions of perfect competition and non-saturation, then the 'narrow' interpretation means that the indifference curves cannot coincide with the price line over any part of its range. This possibility is not very likely, and its inclusion in the theory does make the analysis rather complicated, as we shall see.

bundle contained within this boundary will automatically be rejected in favour of a bundle lying on the boundary. Thus, since if $x^0 \geqslant x^1$, then $p^0 x^0 > p^0 x^1$, we shall say that if

$$p^0 x^0 > p^0 x^1,$$

then x^0 is revealed strictly preferred to x^1 and write $x^0 P x^1$.

Our modification of Samuelson's axiom, which was mentioned above, now reads:

If $x^0 R x^1$, *then not* $x^1 P x^0$,

i.e. if $p^0(x^1 - x^0) \leqslant 0$, *then not* $p^1 x^1 > p^1 x^0$,

i.e. if $p^0(x^1 - x^0) \leqslant 0$, *then* $p^1(x^1 - x^0) \leqslant 0$.

24. If we compare this with Samuelson's weak axiom, we see that it is identical save for the addition of an equals sign in the second inequality. Samuelson's inequality is therefore more restrictive but, by the same token, sharper, so that he is able to deduce theorems in demand analysis that are not obtainable with our present axiom.[1] For our present purposes this is not necessarily a disadvantage. If our axioms are sufficient to establish an ordered system of preferences, then so are Samuelson's complete set (5), which are special cases of ours.

IV. Two Theorems in Revealed Preference Theory

25. We now have our first consistency condition, and it will be interesting to see how far we can go with it alone in proving our desired results.

We shall use it first to establish a theorem that will prove of great use. Before explaining this theorem a little more notation will be required. By a *convex combination* of two commodity bundles x^1 and x^2 I shall mean all those bundles representable in the form $\alpha x^1 + (1-\alpha)x^2$, where α is any real number such that $0 \leqslant \alpha \leqslant 1$. The notation αx^1 means the bundle each of whose components consists of the corresponding component of x^1 multiplied by α. The geometric interpretation of the convex combination of two bundles x^1 and x^2 is the set of all bundles lying along the line segment joining them.

26. We may now state and prove the following:

THEOREM 1. *Given three distinct points* x^0, x^1, x^2 *such that*

$$\textit{not } x^0 P x^1 \quad \textit{and} \quad \textit{not } x^0 P x^2$$

then for no point x^3 *lying on the convex combination of* x^1 *and* x^2 *can we have*

$$x^0 P x^3.$$

[1] It may be noticed that Samuelson [(3), pp. 110–11] obtains his inequality by explicitly assuming that his demand functions are single-valued. This also means the implicit assumption of homogeneity of the demand functions.

27. *Proof.* The theorem is trivially true if $x^1 = x^3$ or $x^2 = x^3$, so that we may take $0 < \alpha < 1$.

The theorem states that if

$$p^0.(x^1-x^0) \geqslant 0 \quad \text{and} \quad p^0.(x^2-x^0) \geqslant 0,$$

then

$$p^0.x^0 \not> p^0.x^3.$$

Let the theorem be false, i.e. let $p^0.x^0 > p^0.x^3$.

Then

$$p^0.(x^0-x^3) > 0.$$

Now

$$\begin{aligned} p^0.(x^0-x^3) &= p^0.\{x^0-\alpha x^1-(1-\alpha)x^2\} \\ &= p^0.(x^0-x^2)+\alpha p^0.(x^2-x^1). \end{aligned}$$

Since $p^0.(x^0-x^2) \leqslant 0$, and $\alpha > 0$,

$$p^0.(x^2-x^1) > 0.$$

Let $\beta = (1-\alpha)$ and consider $p^0.(x^0-x^3)$ again.

We may write

$$\begin{aligned} p^0.(x^0-x^3) &= p^0.\{x^0-(1-\beta)x^1-\beta x^2\} \\ &= p^0.(x^0-x^1)+\beta p^0.(x^1-x^2). \end{aligned}$$

Now

$$p^0.(x^0-x^1) \leqslant 0, \quad \text{and} \quad \beta > 0,$$

so that

$$p^0.(x^1-x^2) > 0,$$

i.e.

$$p^0.(x^2-x^1) < 0.$$

But we have shown above that

$$p^0.(x^2-x^1) > 0.$$

Therefore we have a contradiction by assuming that the theorem is false, and it must therefore be true.

28. It follows from this theorem that the set of all bundles to which a given bundle x^0 is not revealed strictly preferred is convex, and since x^0 is clearly not revealed strictly preferred to itself, the boundary of this convex set must pass through x^0.

29. We can go further than this, however, and with the help of our postulate of non-saturation establish.

THEOREM 2. *Any bundle x^3 lying in the* interior *of the convex set of bundles to which x^0 is not revealed strictly preferred must be such that $x^3 P x^0$.*

30. *Proof.* Let the price vector p^3 associated with x^3 cut the boundary of the convex set in points x^1 and x^2. Since the set is convex, the price line must clearly cut the boundary in two points.[1]

[1] It is not necessary for this proof that there exist only one price vector p^3 associated with x^3. We are not assuming that the demand functions are single-valued, much less reversible.

There are three possible situations:

(*a*) x^0 lies on that part of the boundary of the convex set which lies 'below' the convex combination of x^1 and x^2. Then, by our postulate of non-saturation, it follows that any point on this convex combination, and in particular x^3, must be such that $x^3 P x^0$.

(*b*) x^0 lies on those parts of the boundary that lie 'above' the chord joining x^1 and x^2. But this is impossible, for it would imply that $p^3 x^0 > p^3 x^3$, which would imply that x^0 is actually revealed preferred to x^3, contrary to hypothesis.

(*c*) x^0 is one of the points x^1 or x^2. If this is so, then the convex combination of x^1 and x^2 is actually the boundary of the convex set, for if it were not, then there would be points x^i lying below it, such that $x^0 P x^i$. But this contradicts the definition of the set, and therefore the chord joining x^1 and x^2 must form part of the boundary.

V. A Provisional Definition of Indifference

31. Let us review our progress so far. We have shown that the set of all commodity bundles to which x^0 is not revealed strictly preferred is a convex set whose boundary passes through x^0. We have further shown that all points not on the boundary of this set are actually revealed preferred to x^0. This suggests that all points 'indifferent' to x^0 will be found on the boundary of this convex set. However, we have not yet defined indifference, so that we cannot yet make this step.

32. We accordingly give a provisional definition of indifference which is perhaps intuitively appealing. If $x^0 R x^1$ and $x^1 R x^0$ then we say that x^0 is *indifferent* to x^1 and write $x^0 I x^1$. This corresponds in a natural way to the idea of indifference, for it says that if x^0 is revealed preferred or indifferent to x^1, and x^1 is revealed preferred or indifferent to x^0, we must have that x^0 and x^1 are in fact indifferent.

33. We now have to show that all equivalent batches, if they exist, lie on the boundary of the convex set consisting of bundles to which x^0 is not revealed strictly preferred. We have shown already that they cannot lie in the interior of the set, and it remains to show that they cannot lie in the exterior of the set. This result follows obviously from the definition of the set, for any point, say x^4, in its exterior must be such that $x^0 P x^4$, and this clearly rules out $x^4 I x^0$, since $x^4 I x^0$ implies $x^4 R x^0$, which by our axiom implies not $x^0 P x^4$, contrary to hypothesis.

34. So we have shown that all points that, on our provisional definition of indifference, are indifferent to x^0 lie on the boundary of the convex set.

We can show more than this, however. If follows from our definition of indifference that if $x^0 I x^1$ and $x^1 I x^2$, the following equations hold:

$$p^0 . x^0 = p^0 . x^1 = M^0,$$
$$p^1 . x^0 = p^1 . x^1 = M^1,$$
$$p^1 . x^1 = p^1 . x^2 = M^1,$$
$$p^2 . x^1 = p^2 . x^2 = M^2.$$

Hence $$p^1 . x^0 = p^1 . x^1 = p^1 . x^2.$$

Therefore x^0, x^1, and x^2 are coplanar, i.e. lie on the same hyperplane. Further it follows that $M^0 = M^1 = M^2$, since we cannot have, for example, that if x^0 and x^1 lie on M^0, and also lie on M^1, that M^0 and M^1 are different, since two distinct points cannot lie on two hyperplanes. It follows that $p^0 = p^1 = p^2$.

35. Therefore the points that are indifferent to x^0, on our definition, lie on a 'straight line', i.e. a hyperplane, through x^0. We cannot rule out the possibility that the 'indifference curves' we are after are just the price lines, but such a conclusion is repugnant to common sense, since it would imply that all commodities are perfect substitutes. This reasoning implies that there are points on the boundary curve through x^0 which are not revealed strictly preferred to x^0, nor to which x^0 is revealed strictly preferred, nor to which x^0 is indifferent. They are, in fact, *non-comparable* with x^0. The problem remains of how to deal with them.

VI. A Revised Definition of Indifference

36. In order to do so, a general withdrawal from our rather dangerously exposed positions is required. We shall abandon the previous definition of indifference and substitute a new one. We shall also abandon our version of Samuelson's weak axiom and accept the original version.

37. Our new definition of indifference runs as follows: If x^0 is not revealed preferred to x^1, and x^1 is not revealed preferred to x^0, then we say that x^0 is *indifferent* to x^1 and write $x^0 I x^1$. With this definition it is obvious that we can deal with those bundles that are non-comparable with x^0, mentioned in the last paragraph. But the reverse of the previous problem then crops up, and we now have to deal with the problem of the batches that, on the previous definition of indifference, were indifferent to x^0.

38. We shall deal with this problem by assuming it away. By reverting to Samuelson's weak axiom, we deliberately assume that there are no such equivalent batches. The weak axiom, it will be recalled, stated that, if x^0 and x^1 are distinct and $x^0 R x^1$, then not $x^1 R x^0$. The reason for dealing with this problem in such a cavalier manner is purely for simplicity in exposition. We could retain our version of the weak axiom, and define indifference by

saying that if EITHER x^0Rx^1 and x^1Rx^0, OR $x^0R'x^1$, and $x^1R'x^0$ (where $x^0R'x^1$ reads x^0 is *not* revealed preferred or indifferent to x^1), then x^0 is indifferent to x^1. Theorems precisely analogous to those we are about to prove go through for this type of indifference, but are much more complicated to prove.

39. The present definition of indifference is essentially due to Houthakker [(1) p. 168], for he discovered batches which are neither revealed preferred to a given batch x^0, nor to which x^0 is revealed preferred; and which lie on a convex boundary curve through x^0. He then showed that all such batches are equivalent, in the sense of formal logic, so that we are really entitled to call such batches indifferent. This last result is of crucial importance in revealed preference theory, and Houthakker deserves great credit for being the first to demonstrate it.[1]

40. We may recall the steps of this proof. In order that a binary relation, say I, be an equivalence relation, formal logic rules that it must be shown that the relation is

(*a*) reflexive, i.e. x^0Ix^0 implies x^0Ix^0;

(*b*) symmetric, i.e. x^0Ix^1 implies x^1Ix^0; and

(*c*) transitive, i.e. if x^0Ix^1 and x^1Ix^2, then x^0Ix^2.

[Cf. Tarski (1), pp. 94–96.] Now while it is true that $p^0(x^0-x^0) \leqslant 0$, x^0 is not revealed preferred to x^0, for we were careful to stipulate that the relation R holds only between *distinct* points of the commodity space X. It therefore follows that I is reflexive, and symmetry follows trivially from the definition of indifference. In order to verify (*c*), much harder work is necessary, and we shall merely cite Houthakker's proof, which relies on the 'strong axiom' of revealed preference theory.

41. At this point it will be convenient to state and to adopt Houthakker's axiom, since it will be needed in the proofs of some later results. It is essentially a generalization, to finite sequences of two or more batches, of Samuelson's weak axiom, which it therefore replaces. It may be worded as follows:

AXIOM 2*b*. *For any finite sequence of batches* $x^0, x^1, x^2, \ldots, x^{n-1}, x^n$ *such that* $x^0Rx^1, x^1Rx^2, \ldots, x^{n-1}Rx^n$, *where* $x^0 \neq x^n$, *we have* $x^nR'x^0$.

VII. The Construction of an Ordered Preference System

42. Let us again pause to review our progress. We have shown that, given any commodity bundle x^0, there exists a convex curve through x^0

[1] There is a slight error in Houthakker's proof, which is corrected in Corlett and Newman (1).

such that each bundle lying on it is neither revealed preferred to x^0, nor to which x^0 is revealed preferred. Further, each such batch is equivalent to x^0, which may be expressed in more usual terminology by saying that each such batch is indifferent to x^0.

43. Thus we have constructed a series of equivalence classes—'indifference curves'—and from these we wish to construct a complete preference ordering. In order to do this, we first take a straight line through the origin and a monotonic sequence $x^0, x^1, x^2, \ldots$, of batches on this line, i.e. such that $x^1 \geqslant x^0, x^2 \geqslant x^1, x^3 \geqslant x^2, \ldots$. We then know, by our axiom of non-saturation, that $x^1 P x^0, x^2 P x^1, x^3 P x^2, \ldots$, so that we can order our equivalence classes in ascending order, and assign numbers to them in a way that reflects this order.

44. This is as far as the treatments by Houthakker and by Samuelson (5) take the matter. But the order that has been established *is not a complete preference ordering*. The job of establishing a utility function has not been completed, for we have no algebra yet defined for operating with the relations R and I together. In order to see this, let us suppose that we have two batches x^0 and x^1 such that $x^0 R x^1$, and a further batch x^2 such that $x^1 I x^2$. We cannot conclude, as we would like to and as a complete preference system would enable us to do, that $x^0 R x^2$, for such a conclusion is not warranted by our present system of axioms. We need a further axiom, one which, following the usage of Professor Hicks in a similar matter in his forthcoming book, may be called a 'gearing principle', i.e. a relation that will gear together the two relations R and I.

45. Accordingly we put forward the following additional 'gearing' axiom, which seems reasonably appealing on intuitive grounds.[1]

AXIOM 3. *For any three batches x^0, x^1, and x^2 such that $x^0 R x^1$ and $x^1 I x^2$, we have $x^2 R' x^0$.* In words, this says that if x^0 is revealed preferred to x^1 and x^1 is indifferent to x^2, then x^2 cannot be revealed preferred to x^0.

It follows as a trivial corollary of this axiom, and of the transitivity of indifference that, in fact, if $x^0 R x^1$, and $x^1 I x^2$ then $x^0 R x^2$.

By the adoption of this axiom we have, in a sense, extended the meaning of the phrase 'revealed preferred' in a manner analogous to the extension proposed by Samuelson [(5), p. 370] in making 'revealed preference' a transitive relation.

46. With this axiom and its corollary, plus Houthakker's strong axiom of revealed preference, we may establish a few interesting little results. A first such result is

THEOREM 3. *For any three batches x^0, x^1, and x^2 such that $x^0 R x^1$ and $x^1 R x^2$ we have $x^0 R x^2$.*

[1] The notation $x^2 R' x^0$ reads x^0 is *not* revealed preferred to x^2.

Proof. Suppose the result false. There are two possibilities:

$$(a)\quad x^2 I x^0; \qquad (b)\quad x^2 R x^0.$$

If (a) holds, then we have $x^1 R x^2$ and $x^2 I x^0$, which, by the corollary to Axiom 3, implies $x^1 R x^0$, contrary to hypothesis. If (b) holds, then we have $x^1 R x^2$ and $x^2 R x^0$ which, by Houthakker's axiom, imply $x^0 R' x^1$, contrary to hypothesis.

A trivial corollary of Axiom 3 and Theorem 3 is the following:

THEOREM 4. *For any three batches x^0, x^1, and x^2 such that $x^0 R x^1$ and $x^2 R' x^1$ we have $x^0 R x^2$.*

Proof. If $x^2 R' x^1$ there are two possible cases:

(a) $x^1 R x^2$. Then we have $x^0 R x^1$ and $x^1 R x^2$, and Theorem 3 gives the desired result.

(b) $x^1 I x^2$. The corollary to Axiom 3 applies directly to give the result.

47. A further theorem that is needed is

THEOREM 5. *For any three batches x^0, x^1, and x^2 such that $x^0 I x^1$ and $x^1 R x^2$ we have $x^0 R x^2$.*

Proof. Suppose that the result is false. There are two possible cases:

(a) $x^2 I x^0$. Then $x^1 R x^2$ and $x^2 I x^0$, by the corollary to Axiom 3, give $x^1 R x^0$, contrary to hypothesis.

(b) $x^2 R x^0$. Then $x^1 R x^2$ and $x^2 R x^0$ imply, by Theorem 3, that $x^1 R x^0$, contrary to hypothesis.

48. We are now in a position to complete the establishment of a complete preference ordering with the set of axioms that have been adopted. In order to do so we define a new relation 'revealed preferred or indifferent to'. We say that a batch x^0 is 'revealed preferred or indifferent to' a batch x^1, and write $x^0 R^* x^1$, if either (a) $x^0 R x^1$ or (b) $x^0 I x^1$. Clearly, if $x^0 R^* x^1$, then, by Houthakker's axiom, not $x^1 R x^0$.

49. This relation R^* obviously satisfies the first axiom, mentioned at the beginning of this paper, required for a complete weak ordering, for the relation I is defined to take account of those batches that are not comparable with respect to R. It remains to prove that R^* satisfies the second axiom for a complete weak ordering, and this result we prove as

THEOREM 6. *For any three bundles x^0, x^1, and x^2 such that $x^0 R^* x^1$ and $x^1 R^* x^2$ we have $x^0 R^* x^2$.*

Proof. Suppose that the result is false. Then, by the definition of R^*, we have $x^2 R x^0$. Now since $x^1 R^* x^2$, there are two possible cases:

(i) $x^1 R x^2$ and $x^2 R x^0$. Then Theorem 3 gives $x^1 R x^0$, contrary to hypothesis.

(ii) $x^1 I x^2$ and $x^2 R x^0$. Then Theorem 5 gives $x^1 R x^0$, contrary to hypothesis.

50. Thus the relation R^* induces a complete weak ordering on the commodity space X, and subject to the conditions of Debreu (2), we may define the usual kind of utility function on X. In order to arrive at this result, we have had to make the following definitions and adopt the following axioms:

DEFINITION 1. A batch x^0 is revealed *preferred* to another, distinct, batch x^1 if $p^0(x^1 - x^0) \leqslant 0$. We write $x^0 R x^1$.

DEFINITION 2. A batch x^0 is *revealed strictly preferred* to a batch x^1 if $p^0(x^1 - x^0) < 0$. We write $x^0 P x^1$.

DEFINITION 3. A batch x^0 is *indifferent* to a batch x^1 if not $x^0 R x^1$ and not $x^1 R x^0$. We write $x^0 I x^1$.

DEFINITION 4. A batch x^0 is *revealed preferred or indifferent* to a batch x^1 if either (*a*) $x^0 R x^1$ or (*b*) $x^0 I x^1$. We write $x^0 R^* x^1$.

AXIOM 1. (Non-saturation.) If $x^0 \geqslant x^1$, then x^0 is chosen in preference to x^1.

AXIOM 2*b*. (Houthakker.) *For any finite sequence of batches* $x^0, x^1, x^2, \ldots, x^{n-1}$, x^n *such that* $x^0 R x^1, x^1 R x^2, \ldots, x^{n-1} R x^n$ $(x^0 \neq x^n)$ *then not* $x^n R x^0$. This includes Samuelson's Axiom 2*a* as a special case.

AXIOM 3. (Gearing axiom.) *If* $x^0 R x^1$ *and* $x^1 I x^2$, *then* $x^2 R' x^0$.

VIII. Conclusion

51. It has taken a long time to establish our results, and there is no doubt that they could have been established quicker if the definitions and axioms had been baldly stated right at the beginning. The present axiom system is, of course, not sacred, and objection may be made to some of the axioms. In particular, one may wish to reject Axiom 3, which seems very powerful.

52. (Summary.) Existing treatments have carried the discussion of revealed preference theory to the stage where equivalence classes have been obtained and ranked. The present contribution has surveyed these

developments, and carried them forward, by the addition of a further axiom, to the establishment of a complete preference ordering, and hence to the existence of a utility function.[1]

[1] In his recent paper [May (1)], which appeared after this article was completed, Kenneth May has given an important discussion of the status of the transitivity condition in normal preference theory. The results of his theoretical analysis and of his experiments indicate that transitivity is a much less intuitively obvious assumption than appears at first sight.

OXFORD

APPENDIX

The Problem of Two Commodities

53. As we have already remarked, Samuelson realized explicitly [(3), p. 111, fn. 1] that the weak axiom does not imply that the consumer has a utility function, but since he confused the conditions for a complete ordering with the conditions for integrability, he was naturally led to deduce the existence of a utility function defined over a two-good commodity space, where the integrability condition does not arise. A careful examination of his proof in (4) reveals that he made slips similar to those made by Wold, so that similar criticisms apply. He implicitly assumes that the relation 'revealed worse than' is transitive (cf. (4), pp. 247–8, 'Note too that Z'' is on the budget line of B', and is hence revealed to be inferior to B', which already has been revealed to be worse that A. *It follows that Z'' is worse than A*' [italics mine]) and does not show that all the points lying along the convex boundary curve are equivalent to each other, since equivalence is not defined. Therefore, as in Wold's case, it is not possible to deduce the existence of a utility function.

54. There is in fact very little that is peculiar to the two-dimensional case. However, one theorem we may prove is that Houthakker's axiom is always true in two dimensions if Samuelson's axiom is assumed. More precisely, we have

THEOREM 7. *In the two dimensional case, for any three batches such that x^0Rx^1, x^1Rx^2 we have $x^2R'x^0$.*

Proof. It is sufficient to prove the incompatibility of the six inequalities:

$$\begin{aligned}
p_{11}(x_{21}-x_{11})+p_{12}(x_{22}-x_{12}) &\leqslant 0,\\
p_{21}(x_{21}-x_{11})+p_{22}(x_{22}-x_{12}) &< 0,\\
p_{21}(x_{31}-x_{21})+p_{22}(x_{32}-x_{22}) &\leqslant 0,\\
p_{31}(x_{31}-x_{21})+p_{32}(x_{32}-x_{22}) &< 0,\\
p_{31}(x_{11}-x_{31})+p_{32}(x_{12}-x_{32}) &\leqslant 0,\\
p_{11}(x_{11}-x_{31})+p_{12}(x_{12}-x_{32}) &< 0.
\end{aligned}$$

Now let M_1 be the income when (x_{11}, x_{12}) were bought, and correspondingly for M_2, M_3. Let us now project on to the x_2 axis, and translate the set of inequalities in terms of these projections.

We have $$\frac{M_2}{p_{12}} \leqslant \frac{M_1}{p_{12}},$$

i.e. the (p_{11}, p_{12}) price line through (x_{21}, x_{22}) has to lie within that through (x_{11}, x_{12}), which means that the intercept for the latter must be lower than that for the former.

We have also $$\frac{M_2}{p_{22}} < \frac{M_1}{p_{22}}$$

by Samuelson's axiom.

Similarly, $$\frac{M_3}{p_{23}} \leqslant \frac{M_2}{p_{23}}, \qquad \frac{M_3}{p_{33}} < \frac{M_2}{p_{32}}$$

and $$\frac{M_1}{p_{31}} \leqslant \frac{M_3}{p_{31}}, \qquad \frac{M_1}{p_{12}} < \frac{M_3}{p_{12}}.$$

Since p_{12}, p_{22}, and p_{32} are all positive, these inequalities reduce to

$$M_2 - M_1 < 0$$
$$M_3 - M_2 < 0$$
$$M_1 - M_3 < 0,$$

from which, on adding, $$0 < 0.$$

This is a contradiction, and the inequalities are therefore incompatible, as was to be proved.

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CONCERNING MARGINAL UTILITY

By W. E. ARMSTRONG

Two recent articles in *Economica* have introduced strange twists into the utility controversy. Mr. Bernadelli,[1] who finds utility 'unquestionably ordinal in nature', wishes to rehabilitate 'marginal utility', while Mr. Kennedy[2] would abolish it, though fully accepting my arguments[3] for the determinateness of utility. I agree fully with Mr. Kennedy that marginal utility has not been rehabilitated by Mr. Bernadelli, and Mr. Kennedy's comments reflect my own; and I find myself just as mystified by the mathematical gymnastics[4] in which Mr. Bernadelli indulges. Is it simplicity on my (and presumably Mr. Kennedy's) part to assume that if utility is unquestionably ordinal, as Mr. Bernadelli emphatically asserts, then differentiation of the utility function is meaningless and, therefore, marginal utility a self-contradictory concept?

Mr. Bernadelli's failure, as we see it, to rehabilitate marginal utility, does not however lie in any inadmissability of the concept, and I regretfully part company with Mr. Kennedy when, assuming only the determinateness of the utility function on which we are both agreed, he still finds the concept unacceptable, and I fear I must regard Mr. Kennedy as guilty of one of those 'semantic dodges' about which Professor Robertson[5] gave anticipatory warnings. It should, however, be remembered that Professor Robertson had in mind the avowed ordinalist who retreats to cardinalism so far as preference is concerned and pretends that such preference-cardinalism had never been denied. Mr. Kennedy is not in any sense an ordinalist; what he is doing is openly to raise a new issue, that cardinalism in preference is compatible with abolition of marginal utility. He chides me gently, therefore, as guilty of no more than a slight lapse of caution when I talk of marginal utility. I, at least, he implies, would lose nothing by giving up marginal utility, though the wrench would, no doubt, be greater for Professor Robertson.

Realizing that the question at issue is primarily a logical one, Mr. Kennedy wisely goes to the logicians and what better guide could he have chosen than Bertrand Russell? Using Russell's terminology, Mr. Kennedy finds that utilities have magnitudes, as also have the 'distances'

[1] 'A Rehabilitation of the Classical Theory of Marginal Utility', *Economica*, Aug. 1952.
[2] 'Concerning Utility', ibid, Feb. 1954.
[3] 'The Determinateness of the Utility Function, *E.J.*, Sept. 1939.
[4] According to Mr. Lancaster, 'A Refutation of Mr. Bernadelli', *Economica*, Aug. 1953, there is also something wrong with the mathematics.
[5] *Utility and All That*, Allen & Unwin, 1952.

between utilities; and the latter magnitudes are revealed in the quantitative characteristic of actual preferences. But the magnitudes of utilities are not 'magnitudes of divisibility' (we cannot add or subtract the magnitude-measures of two utilities to obtain a magnitude which is that of the sum or difference of the two utilities), while the magnitudes of 'distance', being magnitudes of a relation, can be treated as magnitudes of divisibility, since 'distances' can be added.[1] Thus a utility, being a determinate distance from any other utility, can be measured (by its distance from a standard utility), even though it cannot be expressed as a quantity of utility; and *a fortiori* the difference between a pair of utilities cannot be regarded as a quantity of utility. In other words, although we can specify any utility uniquely by a number, once we have chosen to indicate a given utility by a number and a given degree of preference by a number, and therefore can speak of a determinate utility function, $u(x, y, ...)$, the difference between two values of this function is not itself a quantity of utility. Therefore, so Mr. Kennedy argues, since $U_1 - U_2$, or $U_1 + U_2$ are not quantities of utility, not only are the operations of addition and subtraction inadmissible, but so also is the operation of differentiation which implies the concept of marginal utility. Now it does, of course, follow that we cannot add a utility, measured by U_1, to a utility, measured by U_2, and obtain in this way a utility, measured by $(U_1 + U_2)$, if utility is intensive in Mr. Kennedy's sense, but that does not mean that we cannot differentiate the function. On the contrary, the rate at which u changes with any of the variables $x, y, ...$, is a perfectly sensible concept. The du of du/dx is the change in the measure of u and does not have to be an increment of utility, requiring the divisibility that is rejected. If I control a light with a rheostat, I can describe the rate of change of intensity with movement of the control at any point in its movement or at any given light

[1] Professor Robbins, though seemingly he accepts cardinality of preference, has doubts about this additiveness. He is prepared to agree that his preference for A over B may be equal in strength to his preference for B over C, without the implication that his preference for A over C is twice that of A over B (or B over C). 'Robertson on Utility and Scope', *Economica*, May 1953, p. 104.

In so far as the contestants in the utility controversy all agree that utility is at least ordinal (i.e. utilities can be ordered lineally, whether or not they also have magnitudes and are separated by 'distances'), there would, I imagine, be general agreement that, if there are also 'distances' between utilities, the 'distance' between U_1 and U_2 is the algebraical sum of the 'distances' between U_1 and U_k and between U_k and U_2, where U_k is any other utility, and these 'distances' measure the strengths of preferences. If utilities are not even ordinal, preferences might behave in a very odd way (just as the spatial distance between A and C of a triangle ABC may be small or large for the same given distances between A and B and between B and C). Although Neumann and Morgenstern in *Theory of Games and Economic Behaviour* hint at the possibility of constructing a utility theory that is not even ordinal, I am content to trust my own introspection that the strength of my preference for A over C has a magnitude equal to the sum of the magnitudes of my preference for A over B and B over C. Experiment mentally as I will I am quite unable to get any other result.

intensity, without implying that a difference of intensity is itself an intensity.

But perhaps Mr. Kennedy wishes to give a meaning to marginal utility that is not merely the $\partial u/\partial x$, $\partial u/\partial y$, ..., of the determinate utility function $u(x, y, ...)$? If so, his criticism of my use of the concept is irrelevant, since with what I now regard as undue, rather than insufficient, caution I assigned no other meaning to marginal utility than rate of change of utility with change of any given consumption variable. I pointed out, moreover, that marginal utilities can be evaluated and utilized for value theory without the necessity either for evaluating total utility or deciding whether or not it has magnitude of divisibility. Nevertheless, Mr. Kennedy has good authority, if he so wishes, for meaning a good deal more by marginal utility than du/dx. Although modern usage is, on the whole, in keeping with the above definition, Marshall[1] uses the phrase 'marginal degree of utility' for du/dx and defines 'marginal utility' as $du/dx.\delta x$. If utility is intensive, the latter expression must mean the increase of intensity of utility brought about by a small increase δx of x (provided that δx is sufficiently small for du/dx to be assumed constant from x to $x+\delta x$) and does not therefore mean the 'utility of δx', though it does measure the strength of preference (or of sub-liminal preference, if there is indifference) for $x+\delta x$ over x. There can, however, be little doubt that Marshall's 'marginal utility', $du/dx.\delta x$, meant for him the 'utility of δx', given x—his doctrine of consumer's surplus would otherwise need reformulating. Mr. Kennedy would, of course, be quite correct in denying that $du/dx.\delta x$ means the utility of δx, if utility is intensive, but that affords no reason for throwing away the concept du/dx. Most of those who today call themselves cardinalists have regarded utility as having intensive magnitude and it is part of the meaning of intensity that it is not 'divisible' and therefore that marginal utility does not mean what Mr. Kennedy seems to think it ought to mean. All that cardinalists need to mean by marginal utility is the way in which utility, measured in the way we have indicated, changes with change of selected variables in the utility function.

Although the validity of the marginal utility concept does not turn on whether or not utility has magnitude of divisibility, so long as it has magnitude, I have recently come to the conclusion that utility should be treated as having magnitude of divisibility, and therefore plead guilty to the crime of which Mr. Kennedy exonerates me. It seems to me now that what looks like 'intensity' is really 'density', and marginal utility can, after all, be allowed to have those additional properties to which Mr. Kennedy so strongly objects. Consider, to begin with, what is involved in an act of 'preferring'. To prefer A to B is, in general, to think of A and B

[1] *Principles of Economics*, 8th ed., Mathematical App. Note 1.

as alternative happenings when in fact neither has happened; and the utility of A and B appears as that quality of A and B, which gives rise to the preference, and is describable as 'liking A' and 'liking B' (to use Mr. Kennedy's apt translation of 'utility') in the sense of liking A (or B) *to happen*. But liking and preference are founded, to a large extent, on actual experience and that characteristic of actual experience which we recognize as 'satisfaction' (or dissatisfaction).[1] Experience (and the satisfaction experienced) always involves duration and is given to us as made up of time-parts. If we divide a given experience into n time-parts, the experience is seen to be composed simply of these n parts (in their temporal order) and the satisfaction of the whole experience is seen to consist simply of the satisfaction of the n parts (without reference to their temporal order). Furthermore the satisfactions of parts or of wholes can clearly be compared as regards their degrees of satisfaction, i.e. I can always judge of two parts that one is more or less or equally satisfactory. Satisfaction has therefore magnitude of divisibility. The satisfaction of one part added to that of another part is without question the satisfaction of both parts.

Let us pursue this matter in somewhat more detail. Consider a toothache, showing varying degrees of painfulness over a period of time. I find I suffer a certain amount of pain over a stretch of 5 minutes, and this is clearly made up of the pain I suffer in each minute of the 5 (it is a simple additive total, independent of the order of the parts, and of the way in which I make the division into parts) even though some of these minutes contain more pain than others. I can also divide the 5 minutes into stretches in such a way as to make the amounts of pain in each stretch equal (and thus with n parts express the pain of the whole as n times the pain of one of these parts). Even though I may speak of variations in the intensity of the pain, where the intensity of pain is thought of as a characteristic of the pain at a moment of time, such variation over a stretch of time creates no difficulty for the making of a judgement as to the amount of pain I suffer during the period. Furthermore, the intensity of pain strictly at a moment is clearly not even a quantity of pain in any sense, for I can only suffer pain in a duration—a cross-section in time of an experience is not an experience; it does not seem to me to be even a 'state' of an experience at a moment, in the sense in which we *can* speak of the 'state' of a physical happening at a moment as an instantaneous snapshot (or even the snapshot together with all the differential coefficients with respect to time of all the constituents) of the physical event. Allow a duration for

[1] It is on the basis of this connexion between wants and satisfactions that Pigou virtually identifies utility, as wanting, with utility as satisfaction, though he draws attention to the many instances of divergence that are also to be found, *Economics of Welfare*, 2nd ed., ch. i.

this intensity, and a quantity of pain emerges; and for a given duration, the quantity of pain is greater the greater the intensity. The question then naturally arises as to whether what I mean by the intensity of pain is not simply the quantity of pain divided by the duration, or, in other words, that so-called pain-intensity is simply pain-density. Perhaps, after all, an intense pain is simply much pain δp crowded into a short duration, δt, i.e. $\delta p/\delta t$, and the apparent intensity at a moment is $\delta p/\delta t$, $\delta t \to 0$, with a somewhat liberal interpretation of $\delta t \to 0$, since there must be a psychological minimum of duration, below which it would be nonsense to speak of 'experience'.[1]

Fortunately it is a matter of no consequence to the economist whether pains and pleasures, satisfactions or dissatisfactions, even enjoyments and disenjoyments varying in some qualitative manner, have intensive magnitude. All that concerns us is that characteristic, whatever it may be, of an experience that leads us to judge our 'liking' of it as compared with other experiences—a characteristic that we also see to be composed of the same characteristic of the temporal parts. As the term utility has already been earmarked for imagined (as contrasted with actual) experiences, I suggest the term 'welfare' for this characteristic of experiences (dropping from the term the connotation good and allowing the proposition 'welfare is a good thing' to be a significant assertion). Welfare, therefore, has 'magnitude of divisibility' in Russell's sense, and even in a wider sense than have such stock examples as mass, owing to the existence of negative wholes and parts. It is clear that the welfare of an experience may be less than the welfare of some of its time-parts. I may be in agony for 5 minutes and reach heights of joy in the next 5. There can be no doubt that the welfare of my total experience is less than that of the last 5 minutes and greater than that of the first. This would be inexplicable unless welfare varied in sign, and requires that the welfare of an experience should be

[1] The concept of density is easily confused with that of intensity and, in the case of experience, it is possible to be in some doubt in certain cases as to whether we should speak of the intensity or density of what is experienced. Light-intensity as experienced seems clearly intensive (a more intense light could hardly be described as more psychical light-stuff concentrated into less duration) but in the case of pain it is possible to doubt whether an acute pain is anything more than much pain concentrated into a short duration. Even if physical pain is intensive (and the case for intensiveness seems stronger for physical pain than for pleasure-pain feeling-tone) we nevertheless have no difficulty in equating as quantities of pain an intense pain of short duration with a less intense pain of longer duration, whereas it seems impossible to equate as quantities of illumination in a psychological sense, an intense light for a short duration with a dim light for a longer duration.

We must also be on our guard against an underlying ambiguity in the concept of psychical density owing to variations of psychical duration with clock-duration. A clock-minute may be a mere moment or almost a lifetime. Density will thus have a different measure according to our measure of duration. But this consideration raises no difficulty—the quantity of pain, of pleasure, of satisfaction, of a given stretch of experience remains an unambiguous quantity, no matter how we describe the density at given points of time.

the algebraical sum[1] of the welfares of the constituent parts. Assuming that my 'preferring' *A* to *B* where *A* and *B* are actual (not imagined) experiences, implies that the welfare of *A* is algebraically greater than the welfare of *B*, welfare emerges as fully cardinal and capable of being positive or negative.[2]

Let us now return to preference and utility, where utility is a characteristic of imagined events and preference a relation between imagined events. Can there be any doubt that when an individual prefers alternative *A* to alternative *B*, it is usually because he believes his welfare, if *A* were to happen, would be greater than if *B* were to happen, and that the utility of *A* and of *B* are simply his estimates of such welfare? Such estimates may, of course, be wrong and they may sometimes be biased. Much behaviour is instinctive and not deliberately chosen as a result of any weighing of alternatives, and much is habitual. Choice on the basis of preference is a highly sophisticated accomplishment and is, no doubt, extremely rare in the animal kingdom. Often my wants take charge and I have but a fleeting glimpse of any but one of a set of alternatives. I want this particular alternative strongly, and only if challenged would I assert that I strongly prefer it to other alternatives, and only if questioned further would I explain how much greater will my welfare be with this alternative than any other I might choose. Such justification may not be genuine—it may well be that I believe my welfare will be greater simply because of the intensity of my want. But I do not find myself able to believe, when influenced by such a want, that my welfare will be smaller. If, through contemplation of the alternatives I *do* arrive at such an estimate, my preferences also undergo appropriate revision. Whatever may be the precise truth on this issue, there is no important problem so far as the field of behaviour which interests the economist is concerned. With few exceptions, utility is simply the individual's estimate of welfare and this estimate can be assumed to be broadly true. In the interests of economy, if not of precision, the one term 'utility' is all we need whether for value theory or for the foundations of 'welfare' theory, and marginal utility can be allowed to mean not merely the differential coefficients of a determinate utility function but the differential coefficients of a total utility

[1] The fact that physical quantities having magnitude of divisibility are rarely regarded as having negative parts should not disturb us and there is, I believe, at least one exception—the charge of an atom is the algebraical sum of the charges of its constituent parts.

[2] In 'Utility and the Theory of Welfare', *O.E.P.*, Oct. 1951, I was somewhat perplexed over the difficulty of defining zero welfare in such a way as to be comparable between individuals. If the welfare of an experience is the same for the whole as for a time-part, it follows that the welfare of the other time-part is zero. With zero welfare defined for the individual then, by means of the concept of marginal preference, total welfare is defined for that individual in a way that makes it comparable with that of any other individual. Thus, as a matter of pure theory, not only do welfare-differences become interpersonally comparable but also absolute welfares.

function, so that the δu of $\delta u/\delta x$, when $u(x, y, ...)$ is the utility function, *is* a quantity of utility.

If, as Mr. Kennedy seems to advocate, we make a distinction between determinate and cardinal utility, the former implying that utility has only intensive magnitude, the latter that utility is quantitative in a divisible sense, then cardinal preference does not imply, but is fully consistent with, cardinal utility. The cardinal preference club welcomes Mr. Kennedy to membership but is suspicious of Mr. Bernadelli's credentials. There is another club, the cardinal utility club, eligible only to members of the above. Its illustrious founders are long since dead and there has been some tightening up of its rules since their day. It is the most select of all utility clubs, if, as I believe, Professor Robertson and A. C. Pigou are members of long standing. I hope Mr. Kennedy may one day be persuaded to join.

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THE TERMS OF TRADE AND THE NATIONAL INCOME 1950–2

By J. R. PARKINSON

THE inflationary boom set off by the Korean War is too recent to need detailed description. There was a large increase in world demand; raw material prices rose to spectacular heights, while prices of manufactures increased more slowly and the terms of trade for many industrialized countries deteriorated in an alarming fashion. In the United Kingdom import prices rose by about 45 per cent. between June 1950 and June 1951 and, for a time, retail prices and wage rates rose at the rate of 1 per cent. per month. A succession of interconnected price changes slowly worked their way through the economy affecting the distribution of income between profits and wages, altering the division of resources between consumption and investment, and reflecting themselves in a serious deficit in the balance of payments before a more stable pattern was established in the economy. The general nature of the relations between the various elements in the national income can be portrayed by the various economic indicators which are available but it is more difficult to analyse the connexions between them in quantitative form. The subject is sufficiently important, however, to warrant the attempt though it may be as well to make it clear at the outset that the statistical data available for the period do not permit it to be done with any accuracy and that the analysis which is developed below contains many elements of guesswork and assumptions of doubtful validity. The starting-point for statistical analysis is the figures of the national income which are published by the Central Statistical Office each year but, since these are compiled only on an annual basis and quarterly figures are not available, it has been necessary to make extensive use of price series and some use of the index of industrial production as an indicator of changes in output and productivity, in order to examine changes during shorter intervals of time. Since it is desirable to bring the picture together by looking at quarterly figures of the national income, an attempt has been made in the Appendixes to interpolate some of the annual figures. This cannot, however, be done very satisfactorily and as the resulting figures can be regarded only as illustrative, they are drawn on only lightly in the text.

1. The first problem is to trace the effects of rising import prices during the period and explain how far the increase in them can be regarded as accounting for the extent and the incidence of the rise in the price-level

of final output. It will be shown on certain assumptions that the rise in import prices and the repercussions following from it were sufficient to account for the rise in output prices which occurred. Before this can be done, however, the assumptions on which the argument rests and a considerable amount of arithmetical guesswork must first be set out.

Market conditions in 1950–2 were far from free: many articles of final consumption were rationed; many raw materials were subject to allocation; and price control of various kinds extended over a wide range of products. Even where price control was not in operation it was no unusual thing to find manufacturers preferring long waiting lists to price increases which would have equated supply and demand in the short-term. It is true that at the other extreme some producers tended to charge what the market would bear, but the general impression of the period is that prices were determined more with an eye to the need for restraint in a period of transition than with the intention of exploiting market possibilities to the full. As a result it is probably at least as useful to regard industry as pricing on a cost plus basis, in the way it had been accustomed to doing during the war, as to regard it as charging prices determined by marginal costs and revenues. If this is conceded prices must be deemed to be tied to costs and to vary with costs in some way.

The term cost is, however, an ambiguous one, particularly in relation to stocks of goods and work in progress. Should the cost of these be reckoned as what was paid for them or as their replacement value at the time they were sold? In other words, should prices be related to current replacement costs or to historical costs?

From the data on profits and stock appreciation given in *National Income and Expenditure 1953* (referred to subsequently as the Blue Book), it is possible to form some idea of the extent to which price determination was based on current and historical costs in 1950 and 1951 and from that to make certain assumptions about the delay between the occurrence of increases in costs and the reflection of them in the price to the final buyer. Before this can be done, however, the relation between figures of profits and stock appreciation must be examined in the light of a theoretical framework.

The accounting method of determining profits is to take the value of sales, subtract from it current expenditure, and add in the difference between the value of stocks and work in progress at the end and at the beginning of the accounting period. The value of these stocks is generally reckoned as being at 'cost or replacement whichever is the lower'.[1] Using

[1] This is now almost a legal requirement. *The Times* Law Report of 12 Dec. reports a case in which Lord Justice Singleton laid down 'as a general proposition' that 'the stocks should be taken either at cost or market price whichever is the lower'.

the following algebraic symbols profits can be described by an equation:

I_s: Value of initial stocks.
F_s: Value of final stocks.
R: Cost of raw materials, fuel, labour, &c., purchased during the period.
S: Value of sales.
P: Profits including stock appreciation.
Px: Profits excluding stock appreciation.

Then $P = S-R+(F_s-I_s)$ and
$Px = S-R$ (subject to the qualifications in the text).

It is now necessary to see how profits and stock appreciation may be expected to behave in periods of changing prices, but in order to do this some rather drastic assumptions must first be made and a simple model considered. The main assumptions are:

1. A smooth and continuous rise in the price of input.
2. An even rate of purchases, production, and sales.
3. No change in the *quantity* of stocks held.
4. For simplicity, that the initial stocks are used completely in the product sold during the period and replaced by new purchases.

In addition some decision must be taken as to how producers determine their profit margin under a cost plus system of pricing. There are a variety of possibilities but the assumption considered here is that the profit margin is a percentage (M) of costs. So far as stocks are concerned, the percentage may be applied either to costs reckoned historically or to costs reckoned currently. Consider first the historical method of price determination. In this case costs comprise the value of the initial stocks (I_s) (since these are assumed to be used in the period considered) and that part of current purchases which is not put into stock ($R-F_s$). If prices are determined by adding a mark up (M) to the sum of these two elements of cost we get:

$$S = I_s+(R-F_s)+M(I_s+R-F_s).$$

The producer will see the first two terms as representing his costs and the second term as representing his profit on the transaction. When, however, profits are determined by deducting the value of purchases, including the value of initial stocks from the value of sales and then adding in the value of final stocks, accounting profits (including stock appreciation) will appear as:

$$P = S-R-I_s+F_s$$

or writing in full $$P = I_s+(R-F_s)+M(I_s+R-F_s)-R-I_s+F_s$$
$$= M(R+I_s-F_s).$$

The difference between I_s and F_s (the difference between the initial and final values of an unchanged quantity of stocks) is numerically equal (although different in sign) to what the national income papers describe as stock appreciation. It will generally not be large in relation to R (the amount expended on purchasing raw materials, labour, &c.) and we may, perhaps, neglect it and regard profits as being equal to MR.

When we consider prices as being determined entirely on the basis of current costs we have: $S = R+M(R)$ where as before the latter term represents profits on the transaction as viewed by the producer. When profits are determined, taking into account the value of initial and final stocks we have:

$$P = S-R-I_s+F_s$$

which in this case gives us: $P = M(R)+(F_s-I_s)$.

We may conclude from this that if in period I prices are constant and in period II they are rising it follows that profits, as defined by accountants, will remain at about the same percentage of purchases if historical costs are charged, but will increase by the amount of stock appreciation if current costs are charged. Alternatively, if it is desired to measure profits excluding stock appreciation, they will fall by the amount of stock appreciation in the first case and remain unchanged in the second.

The movement in profits (including and excluding appreciation) as a percentage of the value of final sales (gross national product plus imports) is shown for a period of years in the table below:

TABLE I

Profits and Stock Appreciation*

	1946	*1947*	*1948*	*1949*	*1950*	*1951*	*1952*	*1953*
	£ million							
Profits including Stock Appreciation .	1,588	1,853	2,023	2,127	2,485	2,881	2,591	2,831
Stock Appreciation .	(100)†	(350)†	242	170	469	545	6	34
Profits excluding Stock Appreciation	1,488	1,503	1,781	1,957	2,016	2,336	2,585	2,797
	Percentage of final sales at market prices							
Profits including Stock Appreciation .	13·9	14·9	14·8	14·5	16·0	15·7	13·7	14·2
Profits excluding Stock Appreciation	13·0	12·1	13·0	13·4	13·0	12·8	13·6	14·0

* Profits of companies, public corporations, and public enterprises.

† No figures of stock appreciation for companies have been published for these years, the figures in brackets are little more than guesses.

It is of particular interest to study the figures for 1949, 1950, and 1951, though it must be remembered that the assumptions made in developing the theoretical framework were not wholly fulfilled during those years. Thus, prices of raw materials and other things rose much more rapidly in the second half of 1950 than in the first, and raw material prices began to fall after the first quarter of 1951. The importance of the fall in raw material prices in 1951 lies in the fact that some firms taking advantage of their taxation option would elect to value their stocks not at cost but at market value. When this is done stock appreciation can no longer be used as a measure of the extent to which historical or current costs are being charged.[1] Nevertheless, although raw material prices fell in 1951, prices of many other things rose and stock appreciation was as great in 1951 as in 1950. A main constituent of the value of stocks and work in progress is wage payments, and wage movements contribute greatly to stock appreciation.

Subject to these reservations, the figures suggest that during the period of rapidly rising costs the pricing policy of producers fell somewhere between the historical and the current cost method. More than that cannot be said with any certainty from the available data.

This conclusion can be interpreted in a variety of ways. It may mean that some adjustment is made to prices immediately an increase in costs occurs, or it may mean that increases in costs are passed on fully but only after some delay. To further the analysis, the rather rash step is taken of assuming that cost increases were passed on fully but only after a delay of half the relevant production period.

The period of production in which we are interested is of the general type defined as the relationship between output, on the one hand, and stocks and work in progress on the other. For the economy as a whole, the value of final output (gross national product at market prices plus imports) amounted to roughly £15½ milliard in 1950 and this was backed by stocks and work in progress of about £5¾ milliard. This suggests that the period of production for the economy as a whole was about 4½ months. This is, of course, only an average figure and there are likely to be considerable variations for different types of input and output. No less, the average period of production does not tell us how long ago production commenced but only how long on the average the various items of input have been in the course of production. Nor do figures which include taxes necessarily give any accurate indication of even the average period of production in a physical sense; the levying of indirect taxes, for example, on consumers who do not hold stocks tends to swell the value of gross

[1] Considerable complications are also introduced if the method of costing is on a 'last in first out' basis rather than on 'first in first out'.

output without altering stock levels or affecting the period of production.[1]

It is useful to see how the period of production varies in the various industries and from the data in the Blue Book it is possible to compile a table for a limited number of them.

TABLE II

Stocks and Work in Progress in Relation to Output in 1950

	Average of value of stocks and work in progress at the end of 1949 and 1950 (1)	*Value of total output in 1950* (2)	*Estimated period of production Col.* (1) × 12 + *Col.* (2)
	£ million		Months
1. Agriculture and forestry	519	1,118	5½
2. Mining and quarrying	69	571	1½
3. Manufacturing industry:			
(a) Metals and engineering	1,186	3,190	4½
(b) Textiles, leather, and clothing	514	1,650	4
(c) Food, drink, and tobacco	345	2,517	1½
(d) Other manufacturing	259	2,057	1½
4. Electricity, gas, and water	90	542	2

Now if the input into each of these industries consisted solely of directly purchased factors of production such as imports or labour, and if all the output of the industries were sold directly to consumers, it would be relatively easy to decide, with the aid of assumptions about the determination of the pricing policy of producers, how quickly cost increases would be passed on to the final consumer. In the case of agriculture, for example, we could argue that cost increases would be passed on in half the production period or about 3 months, and so on. In practice, however, the production flows are much more complicated. Some part of the input of the various industries is derived from other industries and their output is sold, not directly to final consumers, but, through the intermediary of wholesalers and retailers, and part of output also is fed back into the production chain. A very complete analysis to take account of all these factors is not beyond the bounds of possibility, but for it to be done satisfactorily, a mass of detailed statistics would be needed. Some progress can, however, be made by means of drastic simplifications and it is in terms of these that analysis must proceed.

[1] Excluding indirect taxes and subsidies from the value of final output gives a figure of about 5 months for the period of production.

First, let us consider imports in order to see what simplifications are reasonable for this type of input.

TABLE III

Import Content of Final Output

	Direct import content	*Import content of final output*			*Proportion of output sold to final users*
		Direct	*Indirect*	*Total*	
	£ million	Percentages			Percentages
Agriculture . . .	53	5	6	11	42
Mining and quarrying .	11	2	3	5	25
Metal engineering, &c. .	218	7	4	11	75
Textiles, leather, and clothing	468	28	3	31	78
Food, drink, and tobacco .	396	16	4	20	94
Other manufacturing . .	393	19	4	23	37
Building and contracting .	39	3	6	9	80
Electricity, gas, and water .	1	..	4	4	55
Other production and trade	210	4	2	6	77

The indirect import content which can be derived from Table 15 of the Blue Book shows for any one industry how much of its input from other industries was composed originally of imports. It is generally fairly small in absolute amount and we shall probably not go too far wrong if we neglect this element and concentrate attention on that part of the output of individual industries which is not sold through wholesalers and retailers to final consumers, but which instead is fed back into the production chain as a constituent of input—indeed by doing this, we do in fact take account of the neglected element. This procedure enables us to divide import input into two streams: the first is supposed to go through industry directly and on to the final consumer; the second is supposed to be fed back into industry and find its way once again through the production chain. There is a danger that this will lead us into considering an infinite number of progressions through the production chain. We must, however, draw the line somewhere, and it seems most convenient to act on the supposition that for any industry the direct import input multiplied by the proportion of output sold to final consumers gives the amount of imports going once through the production chain, and that the remaining import input can be considered as going twice through the production chain. This is no doubt a very rough and ready procedure, but it appears to fit in with the facts quite well, as will be shown later.

Not all imports, however, are processed by industry; some are purchased by final buyers through the intermediary only of wholesalers and retailers.

How long this process takes cannot be gleaned from the available statistics and the assumption has been adopted that the process takes about 2 months.

Finally, another factor peculiar to imports must be taken into account. This is that the relevant production periods for imports are different from those for other types of input. The main difficulty here lies in the fact that stocks of imported raw materials frequently amount to 6 months' consumption and the production period for these must be longer than the average of all types of input.

The difficulty in arriving by elementary means at the relevant production period for imports of different kinds is now only too apparent, and the above discussion should, perhaps, only be regarded as a slight justification for the adoption of the assumptions about the production period of imports set out in the table below.

TABLE IV

Time taken for Imports to reach Final Consumers

	Imports in 1950 £ million	*Period of production in months*			
		As unprocessed stocks	*In processing*	*With wholesalers and retailers*	*Total*
Unprocessed imports going to final buyer	850	..	..	2	2
Imports processed by only one industry (excluding food, drink, &c.)	900	6	3	2	11
Imports processed by two or more industries	525	6	6	2	14
Food, drink, and tobacco imports sold to final buyers	375	..	2	2	4

The first figure in the first column is extracted from Table 15 of the Blue Book; the second figure is compounded of the import input to the various industries multiplied by the percentage of output going to final buyers; the third figure is the amount of imports which is not regarded as going to final buyers but which is assumed to be fed back into the production chain; and the fourth figure is that part of the import input of the food, drink, and tobacco industries which is regarded as going straight to the final buyer. The figure of 3 months production time for once processed imports is an average for the various industries weighted by their import input; and the figure for twice processed imports has been assumed to be double

the average for once processed imports. The time taken for imports to pass through the hands of wholesalers and retailers is assumed to be 2 months.

If import costs were passed on to final buyers only at the end of the production period, a considerable time would elapse before import price changes were felt fully by final buyers. Cost increases are passed on with some delay, but in a much shorter time than the total production period for imports. For all kinds of input, and over the economy as a whole, it appears reasonable to suppose that costs are passed on in about half the production span, but this is unlikely to be true at all stages of production, or of all types of input. Where the proportion of import input in stocks is high—at the raw material end—prices are at their most flexible. It is not the practice of those who make their living from price fluctuations, or whose living is imperilled by them, to hesitate to pass on increased costs immediately. Thus, in the wool textile industry where the value of imported wool bulks large, it is to be expected that manufacturers will work much more to current than to historical costs, and that prices of such intermediate products as tops and yarn will follow closely, and with little interval, the price of raw wool. It is reasonable to suppose that the price of raw material stocks will vary closely with import prices and cost increases at this stage of production can be regarded as being passed on immediately. At the wholesale and retail end of the production process, imports will form a smaller percentage of the value of sales and here it appears reasonable to suppose that costs are passed on with a delay about equal to the period of production. In between these extremes, where the import content is neither very high nor very low, it appears reasonable to suppose that costs will be passed on in half the production period. Considerations of this kind suggest that for imports going straight to the final buyer cost increases will be passed on in 2 months, that for once processed imports the appropriate delay will be a little less than 4 months, for twice processed imports 5 months, and for food imports 3 months.

What can be surmised as to the delay in passing on increases in wages and salaries? The kind of statistical problems arising here are similar in kind to those discussed in the case of imports. The output of one industry is the input of another and some wage increases have to percolate through several industries before they reach the final buyer. Strictly speaking, we ought to examine each item of wage input separately and pursue it through the whole production process, or at least treat wage input in the same way as imports. A somewhat more summary treatment may, however, suffice. As for imports, we can estimate the amounts of directly incurred wage costs which can be considered as incorporated in final output and obtain a weighted average of the period of production of the various industries; and, a similar very rough estimate can be constructed

for wage input which is fed back into the production process on the assumption that this will roughly double the production period; these two estimates can then be combined to form a single production period. A calculation of this kind suggests that the production period for labour input, including wholesale and retail transactions, is about 4½ months and, on the assumption that costs will be passed on in half the production period, prices to the final buyer are likely to be adjusted after an interval of a little more than 2 months.

2. We now have some working assumptions about the time taken for import and wage costs to be passed on to the final buyer. The next step in the argument is to see whether they can be used to provide a reasonable explanation of the change in final output prices which followed the Korean War.

Before this can be done, a monthly index of final output prices must be constructed. The method of interpolation followed is described in the Appendix I and the results are shown in column 4 of Table V.

The first column in the same table shows the effect on final prices of a change in import prices taking place in previous months. It is compounded of the effect of changes in the price of manufactured articles assumed to have effect 2 months after they took place, of changes in food, drink, and tobacco prices effective in 3 months, and of raw material prices effective in 4 and 5 months respectively in the proportion of roughly 2 to 1, corresponding to the value of raw materials once and more than once processed. The price indices used were those published each month by the Board of Trade. The second column shows the effect of wage increases assumed to affect prices 3 months[1] later, and column (3) cumulates the results. In 1950 wages and salaries accounted for one-half of the value of final sales and imports for about one-fifth, and this has, of course, been taken into account in determining the effect of wages and imports on the price level of final output.

The agreement between columns (3) and (4) is reasonably close though it is noticeable that in the second half of 1951 the price index constructed from the model was rising more rapidly than appears to have been the case, in fact, judging from the interpolated figures. Apart from the nature of the calculations involved, some discrepancy between the two sets of figures might be expected, for the model takes no account of variations in productivity and makes no allowance for changes in profit margins. Over the period as a whole there was an appreciable rise in productivity; between

[1] The wage index is compiled for the end of the month to which it refers whereas import prices are an average for the month. Since the delay assumed in passing on wage increases is a little more than 2 months, wage increases at the end of a month will become effective about the middle of the third following month.

TABLE V

Prices of Final Output reconstructed from Import Prices and Wages, June 1950–June 1952

	Effect of import prices on final output prices (*1*)	*Effect of wage changes on final output prices* (*2*)	*Joint effect cumulated* (*3*)	*Interpolated final output prices* (*4*)
	Figures of percentage change from June 1950			
1950				
July	0·4	..	0·4	..
August	0·4	..	0·8	..
September	0·1	..	0·9	..
October	0·2	..	1·1	1
November	0·4	..	1·5	2
December	0·4	..	1·9	2
1951				
January	0·6	0·5	3·0	4
February	0·8	1·0	4·8	5
March	0·8	0·5	6·1	6
April	0·6	0·5	7·2	8
May	1·3	0·5	9·0	10
June	1·4	0·5	10·9	11
July	1·3	0·5	12·7	13
August	1·4	..	14·1	14
September	1·2	0·5	15·8	14
October	0·7	0·5	17·0	15
November	−0·2	..	16·8	16
December	−0·9	0·5	16·4	17
1952				
January	−0·3	0·5	16·6	18
February	−1·2	2·0	17·4	18
March	−0·5	0·5	17·4	18
April	+0·3	..	17·7	19
May	+0·1	0·5	18·3	19
June	+0·2	0·5	19·0	20

1950 and 1951 the volume of output increased by some 3–4 per cent., most of which was due to higher productivity; on the other hand, there was a rise in profits (excluding stock appreciation) between the same years of about 15 per cent., and the net effect of these opposite movements on prices is unlikely to have been large. It would, however, be unwise to read too much into the figures. The most that they can show is that the assumptions on which the model has been built are reasonably consistent with what took place; it is always possible that other assumptions would lead to the same result.

In the years 1950–2 hourly earnings seem to have moved very closely with wage rates, contrary to experience in earlier years, and there is no reason for our purposes to consider one index rather than another. Over the same period it is also true that there was a very close resemblance between movements in the index of retail prices and wage rates. Although in both the summer of 1951 and 1952, wage rates did not rise as quickly as the index of retail prices, over the 3 years real wages as defined by dividing one index by the other varied by only 1–2 per cent. As a first approximation there is justification, therefore, for assuming that real wages were constant during the period. This is what we should expect to happen in conditions similar to those of 1950–1: in the prosperity of booming inflation labour is in a favourable position for advancing wage claims; on the other hand, with deteriorating terms of trade the nation as a whole is becoming poorer and there will be a natural tendency for producers to pass on increased costs in the form of increased prices and so prevent a rise in real wages. On the whole, the impression gained from looking at the data of the period is that an increase in retail prices was met by an almost simultaneous rise in wage rates.

At first sight this judgement appears to be contrary to the findings of Messrs. Peacock and Ryan who have shown that a very considerable time lag exists between a rise in the cost of living and the presentation, to say nothing of the satisfaction, of wage claims.[1] Nevertheless there may be no real contradiction involved. It is not to be supposed, of course, that a change in wages is a consequence of a change in retail prices taking place during the month but merely that over a period of time the two will tend to keep pace. This is quite consistent with a considerable length of time being taken to satisfy wage claims. What happens in practice is that wage claims are almost constantly under consideration; the settlement of a claim for one industry is the sign for the presentation of new claims from other industries, and often for the settlement of other claims which have been outstanding. If all outstanding claims were settled simultaneously, wages would for a time gain very considerably. Claims are, however, settled piecemeal and it may be surmised that in times of rising prices claims come to fruition more quickly than would otherwise be the case and, moreover, that as settlements tend to be related to recent changes in retail prices, they are the more favourable to wage earners. Thus, there is nothing inconsistent between wage claims being settled only with considerable delay and the wage index and retail prices keeping in step.

The delays and the magnitudes of the price increases constructed in the model are, of course, valid for final output as a whole and are not applicable as they stand to the components of final output such as con-

[1] *Economic Journal*, June 1953, p. 385.

sumption, investment, or exports, which include varying proportions of imports and wage costs in their composition and give more or less weight to the various industries. The annual price indices for these various types of output which are set out in Appendix I, show that their price movements differed appreciably. Exports increased in price between 1950 and 1951 by 17 per cent., fixed capital formation by 9 per cent., and consumption by 9 per cent. Yet for all these differences there can be little doubt that the rise in import costs was the prime mover in the rise in consumption prices which took place and from which followed the rise in wages. This suggests that we shall not be very far wrong in concluding that increases in import prices affected final output and consumption prices very rapidly, probably in a typical instance about 3 months after they were recorded in the trade and navigation accounts; a consequent rise in retail prices was immediately followed by an almost corresponding rise in wages affecting prices once again with a delay of about 3 months. In this way it appears probable that the whole of the increase in the price of final output between mid-1950 and mid-1952 can be attributed directly or indirectly to changes in import costs, and if the course of import prices could have been predicted, so could the course of final output prices.

It would, of course, be unwise to assume that the kind of relationships which appear to have been valid for the Korean inflation would also be valid in different circumstances. It is also most unlikely that a fall in import prices would work in a symmetrical manner; it is very difficult to conceive of wages in any but exceptional industries moving downwards in response to a fall in the cost of living, and it is by no means as certain that producers are as willing to pass on reductions in costs as they are to delay price increases. Nevertheless, it may be surmised that producers in the United Kingdom are unlikely to depart radically from their established practices of relating selling prices to costs or of passing on cost increases only after some delay.

By the beginning of 1952 the fall in import prices, which had taken place in the second half of 1951, was working in the direction of reducing final prices, but wage increases taking place before the turn of the year had a roughly balancing effect. At this time the upward movement of prices was in course of flattening out, the price inflation was subsiding and comparative stability could be expected.

In the second half of 1952 import prices could no longer be regarded as the prime mover in the determination of prices throughout the economy and although they continued to fall they did not, of course, lead to a reduction in wage rates or to a fall in the price of final output. The simple model of price formation developed above cannot be applied to the conditions of the latter part of 1952: the reduction of subsidies became an

important factor in wage bargaining; the sellers' market had disappeared for a variety of products; and producers modified the methods of price determination that they had applied in 1950 and 1951.

3. The price movements occurring during the period 1950–2 carried with them some interesting changes in the distribution of income between wage and profit incomes. The deterioration in the terms of trade meant a loss of real income to the United Kingdom, but against this could be set after a time an increase in production and productivity. Real wages remained fairly constant during the period and the bulk of income changes resulting from fluctuations in the terms of trade and production can be regarded as reflected in non-wage incomes. Changes of this nature are masked in the annual national income accounts partly because they were transient and partly because profits were alternately inflated and deflated by stock appreciation. The nature of the change in the distribution of income can be brought out only by considering figures at quarterly or shorter intervals, and an attempt has been made in the Appendix to concoct quarterly figures of wage and non-wage incomes by interpolating the annual figures. The interpolations must be regarded with the greatest reserve, but they may serve to illustrate the changes in the distribution of income which occurred. Non-wage incomes cover rent and the income of the self-employed as well as profits and they exclude stock appreciation.

TABLE VI

Distribution of the National Income between Wage and Non-wage Incomes

	Wage incomes %	*Non-wage incomes* %
1950		
1st Quarter . .	66½	33½
2nd „ . .	66½	33½
3rd „ . .	66½	33½
4th „ . .	68	32
1951		
1st „ . .	69	31
2nd „ . .	67½	32½
3rd „ . .	66	34
4th „ . .	67	33
1952		
1st „ . .	66	34
2nd „ . .	66	34
3rd „ . .	65½	34½
4th „ . .	66½	33½

The method of interpolation used has been devised so as to smooth out seasonal variations and the changes in the table are not affected therefore by temporary changes in production or holidays. The share of non-wage earners fell in the fourth quarter of 1950 and there was a further fall in the first quarter of 1951; thereafter there was a recovery and in 1952 non-wage earners appear to have got a somewhat greater share of the national income than they obtained in the first half of 1950. Production rose rapidly in the latter part of 1950 and in the third quarter of that year it was about £75 million greater than the average of the first half; in the first quarter of 1951 there was a further increase of about the same amount which was maintained until about the second quarter of 1952. The increases were not, however, sufficient fully to compensate non-wage incomes against the loss caused to them through passing on increasing costs only after some delay, and it was only when import prices began to fall that non-wage incomes recovered their position relative to wage incomes.

The balance of payments was also affected by the delay in passing on increases in costs which was not confined to home sales but extended to the export market as well, though possibly to a lesser extent. The overseas sellers' market was not exploited to the full. Indeed the delay in passing on cost increases had the effect of keeping the price level of the gross domestic product as a whole (though not necessarily of that portion going to exports) virtually constant until the second quarter of 1951 in spite of the large increase in import prices and wage increases.

In the long run, the income effects of changes in the terms of trade should help to offset their immediate effect on the balance of payments. The incomes of countries benefiting from a change in the terms of trade rise while the incomes of those losing fall. As a consequence the latter group may be expected to reduce their real expenditure while the former may be expected to increase theirs. Restoration of equilibrium in a balance of payments disturbed by changes in the terms of trade is likely to be a complicated and lengthy business. The ease with which it is brought about will depend on a variety of factors including the availability of surplus capacity in a country experiencing a deterioration in the terms of trade, the position of its competitors in international trade, and the extent and the speed of the translation of increased incomes in countries which gain into effective demand for imports. It is not of course to be expected that the various reactions to changed terms of trade will be independent of the way in which the change is brought about. A combination of import and export prices rising at different rates may be expected to have different effects from a combination of falling prices or a mixture of movements opposite in direction.

In 1950–1 the effect of the deterioration in the United Kingdom's terms of trade was to reduce profit incomes. Wages remained relatively untouched (though in other circumstances they might have increased) while profits fell. This did not, however, effect any immediate net reduction in expenditure in the United Kingdom. In the first place the reduction in profits (excluding stock appreciation) was not obvious because it was masked by a rise in stock appreciation. Indeed most companies must have been under the impression that profits were increasing rapidly. In the second place though profit margins (excluding stock appreciation) were falling and the cash position of companies becoming correspondingly worse they either had adequate financial reserves or were able to borrow to meet their needs. A cut in current income did not therefore give rise to a cut in current expenditure as might perhaps have been the case if wage incomes had felt the full impact of the changed terms of trade.

In total, home demand far from decreasing increased markedly for the simple reason that expenditure in total exceeded domestic incomes. At its worst the deterioration in the terms of trade cost some £500 million; in addition government expenditure rose in constant (1948) prices, by some £400 million between mid-1950 and end-1952. And stockbuilding in 1951 was of the order of £600 million greater than in 1950. These additional charges were not all made on the economy at the same time. Had they been so a much more violent inflation than actually occurred would have resulted. It seems probable, however, that increased demands in 1951 exceeded throughout the year the increase in output which had occurred since mid-1950. It was not till 1952 when the terms of trade were receding towards their pre-Korean level and destocking was replacing stockpiling that demands and resources were brought into line. In the circumstances of 1951 it was hardly to be expected that exports could have increased sufficiently to restore a balance on overseas account. Even if demand in total could have been reduced sufficiently, and this must be regarded as unlikely, it would have been difficult to free the right kind of resources for export.

There can, however, be little doubt that the demand for U.K. exports increased greatly. There was a spontaneous increase in demand overseas arising from the expectation of shortages and defence expenditure and, after some delay, the increase in the incomes of producers and exporters of primary products began to generate increased demands for imports. We cannot, of course, be sure that the increase in demand for imports by primary producers would have been sufficient to have brought the U.K. balance of payments into equilibrium even if an adequate volume of exports could have been made available and it is extremely difficult to judge what lag occurs between an increase in income arising from a change

in the terms of trade and the effect of that increase in the demand for imports.

If we turn to the experience of the U.K. in the inter-war period and look at figures of the terms of trade and the volume of exports, we find that a deterioration in the U.K.s terms of trade was generally associated with a larger (percentage) rise in the volume of U.K. exports. This suggests that had the U.K. been able to free productive resources for export in 1950–1 equilibrium could have been restored even if no subsequent improvement in the terms of trade had developed. It is doubtful, however, if we can go so far as this on the strength of inter-war experience. In the inter-war period the overseas account was kept in rough balance at the expense of unemployment. When activity increased imports increased and since the change in invisible earnings was small, exports increased by a larger percentage than the deterioration in the terms of trade. From evidence of this kind it is difficult to be sure that the deterioration in the terms of trade in 1950–1 would, of itself, have given rise to a more than corresponding rise in the volume of exports. Nevertheless, post-war experience cannot be wholly divorced from pre-war; after Korea there were many of the familiar signs of boom and slump; an increase in activity was associated with an increase in exports and a deterioration in the terms of trade; and the improvement in the terms of trade in 1952 and 1953 was accompanied by a fall in activity. And, more important, the increase in the imports of primary producing countries, however caused, was certainly very large.

TABLE VII

*Current Value Quantum and Unit Value of Imports and Exports of selected Primary Producing Countries**

	1950 Second half	*1951 First half*	*1951 Second half*	*1952 First half*
	First half 1950 = 100			
Imports				
Current value . .	120	146	167	166
Quantum . . .	113	124	134	132
Unit value . .	106	118	125	126
Exports				
Current value . .	129	156	139	136
Quantum . . .	111	113	103	108
Unit value . .	115	138	135	126

* Source: *United Nations World Economic Report 1951–2*, Tables 65 and 72.

Though ultimately the income and substitution effects of the deterioration in the terms of trade might have led to an increase in exports sufficient to balance the overseas account, an appreciable time lag occurred between the change in the terms of trade and its effect on the imports of primary producers. Although peak export earnings occurred in the first half of 1951 it was not until the second half of that year that imports reached a peak which was maintained into the second half of 1952. This delay was, no doubt, affected by the circumstances of the time but some delay is always to be expected and the effect of these delays on the balance of payments of the U.K. may be very considerable. As a financial centre the U.K. frequently pays for its imports in advance of receiving them and frequently supplies exports against future payment. If there is a delay of 6 months between primary producing countries receiving higher prices for their exports and spending the proceeds on imports, the restoration of equilibrium in the balance of payments of the U.K. after a deterioration in the terms of trade will take a considerable time, perhaps a year, even though equilibrium is in process of being restored and the underlying situation as revealed by the actual movement of goods and services is much less extreme than the payments position.

Fluctuations in the terms of trade as violent as those in 1950–2 are hardly likely to be a recurring feature of the U.K.s economy. It was the speed and magnitude of the changes in the terms of trade coupled with a large simultaneous increase in home demand which led to the serious external difficulties which developed at that time. There is less to fear in the long run from a more gradual deterioration in the terms of trade since its effects on income tend to be self-righting. Overseas some considerable delay is to be expected before improved terms of trade are reflected in higher imports. In the U.K. the process of freeing resources for export will be slowed if prices are closely related to historical costs and the loss of real income attendant on a deterioration in the terms of trade falls on non-wage earners and does not lead immediately to a cut in home demand. But instantaneous reactions to changing circumstances are not to be expected in the economic world, and what matters in the long run is that they should not be too long delayed.

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APPENDIX I

Interpolation of Final Output Prices

TABLE 11 of the Blue Book gives expenditure at 1948 factor costs. Factor cost expenditure at current prices can also be computed from Table 1, and Table 41, which allocates taxes on expenditure and subsidies by type of expenditure. The

annual average price changes for the various categories of expenditure implied in the constant price figures can thus be calculated:

Price Indices of Final Output at Factor Cost

1948 = 100

	1950	*1951*	*1952*	*1953*
Consumers' expenditure	105	115	122	124
Public Authorities' expenditure . . .	109	119	126	131
Gross fixed domestic capital formation . .	107	117	130	131
Exports of goods and services . . .	109	127	134	128
Final output (including stock formation) . .	107	119	126	127
Imports of goods and services . . .	117	148	144	131
Gross domestic product	104	112	122	126

The price indices for consumers' expenditure can be interpolated with the aid of figures of quarterly expenditure at constant and current prices published in the *Monthly Digest of Statistics* and the monthly index of retail prices. Price indices for public authority expenditure cannot be interpolated so readily but the movement was much the same from year to year and the wage index was taken as a guide in carrying out the interpolation. Interpolation of the prices for gross fixed domestic capital formation is difficult but movements in wholesale prices of building materials and products of the electrical and mechanical industries were taken as a guide in filling out the figures. Interpolation of export and import price indices also presents considerable problems, particularly because no monthly price indices are available for invisibles. A good deal of judgement had to be exercised in making the interpolation which may lead to appreciable error in the final result. No attempt was made to deal with the price of the *change* in the level of stocks which took place.

When the separate interpolations had been completed they were combined with the use of suitable weights into composite indices applicable to final output and the gross domestic product and the annual series for these totals were then interpolated.

APPENDIX II

Interpolation of the Gross Domestic Product

CHANGES in the gross domestic product at current prices are due to changes in prices and changes in the real value of output. If quarterly indices of both these elements can be prepared they can be combined and used to interpolate annual figures of the gross national product. A method by which quarterly indices of price changes can be compiled has been outlined in Appendix I. It remains to outline the construction of a companion index of changes in real output.

Table 12 in the Blue Book gives indices of the contribution to the gross national product of various industries measured at 1948 prices. The output of those industries which are covered by the index of industrial production can be interpolated fairly readily. Since it is advantageous to eliminate seasonal variations the interpolation was carried out using the index corrected for seasonal variations published in the *London and Cambridge Economic Service*. It is more difficult to devise satisfactory methods of interpolating the output of other sectors but for most of them the yearly changes were either small or suggestive of a straight line trend and it seemed reasonable to assume that the output of distribution followed the pattern of the index of industrial production. The figures for the various sectors were then weighted to form an index of the quarterly changes in output for industry as a whole and combined

with the index of price changes to give an index of the quarterly change in the value of output. This composite index was then used to interpolate the annual figures at current prices of the gross domestic product.

In order to divide the gross national product between wage and salary and non-wage incomes the wages bill was also interpolated using the figures of employment published by the Ministry of Labour and the monthly index of wage rates. The result of these computations is shown in the following table.

	Gross domestic product	*Wage and salary incomes*	*Non-wage incomes*
		£ billion	
1950			
1st Quarter	2,790	1,850	940
2nd „	2,790	1,860	930
3rd „	2,800	1,860	940
4th „	2,810	1,910	900
1951			
1st „	2,920	2,020	900
2nd „	3,070	2,070	1,000
3rd „	3,210	2,110	1,100
4th „	3,270	2,190	1,080
1952			
1st „	3,340	2,200	1,140
2nd „	3,380	2,220	1,160
3rd „	3,420	2,240	1,180
4th „	3,450	2,290	1,160

MULTIPLE-COMPANY MERGERS AND THE THEORY OF THE FIRM[1]

By DAVID SCHWARTZMAN

THEORIES of the firm, whether they are of the monopolistic or competitive species, assume that an enterprise or a group of enterprises under common control is a unit with a single purpose, maximization of the profits earned by the unit. It has occurred to some authors that a firm is a grouping of interests and that its decisions do not necessarily reflect any single objective, whether it be maximum profits, maximum earnings to management, or maximum wages. Nevertheless, the theory of profit-maximization is not far wrong when it is applied to most decisions that may be made by a single-company firm. To survive an independent enterprise must earn profits. It is not likely to invest in projects which promise a lower rate of return than others. Nor will it pay more for its materials and labour than the prevalent market prices. When, however, an enterprise is part of a merger, profits are no longer a condition of its existence, and other considerations may dominate. An unprofitable plant may be supported out of the earnings of affiliated enterprises, and the controlling group may be more interested in that plant than in the others.

The problem of the relationship between parts of mergers raises the questions of the limits of the firm. The firm is commonly defined as any independent decision-making unit. On this basis we would expect a merger to maximize the sum of the profits of the enterprises within it. However, even though two plants may be under a single controlling group, their joint profits are not necessarily an important index of achievement. On the other hand, the emphasis which is given to profits in most analyses of firm behaviour suggests that stockholders' interests rather than the extent of control provide a basis for distinguishing firms. Thus if the capital structure of a merger establishes distinct interests associated with each of the enterprises under its control one might consider it to be several firms. Stockholders in any one of the enterprises would expect its management to favour it as opposed to any of the affiliated enterprises. A controlling group may nevertheless neglect the welfare of particular groups of shareholders. It may be primarily concerned with the total profits of the merger, and joint profit-maximization does not necessarily lead to the same results as independent profit-maximization. If decisions are based on the merger's total profits, prices on intra-merger shipments may be arbitrary, and independent profit-maximization may arrive at market-determined prices.

[1] This study is based on a doctoral dissertation supervised by Joe S. Bain. I am also indebted to J. C. Weldon for a critical reading of an early draft of the manuscript.

Alternatively, because of differences between its interests and those of other groups, a controlling group may be more concerned with the profits of a single enterprise than with those of the merger as a whole, and individual enterprises may be prevented from operating independently.

Attention has been given to the problem of the protection of the interests of shareholders who are not members of a controlling group.[1] The primary concern of authors in this field has been the defence of the small stockholder, but such exploitation has its limits even if they are only moral ones. The object of this essay is to show that in any multiple-company merger where different groups of shareholders are attached to each of the companies we may expect certain of the companies to be favoured, without the primary objective being the exploitation of weak shareholders.

I propose to examine the behaviour of a particular merger in order to determine whether it was directed at profit-maximization. In view of the question of the limits of the firm, it will be necessary to inquire: (1) whether the merger maximized total profits, or (2) whether the parts maximized their profits separately. It will be found that neither the merger nor its members maximized profits. The explanation of the observed behaviour will be in terms of the legal relationships between the separate interests in the merger.

The merger to be investigated is the Dominion Steel and Coal Corporation (Dosco), one of the largest coal producers in North America and one of the three integrated steel producers in Canada. The general plan will be (1) to construct two alternative hypotheses based on the profit-maximization model and to specify the data necessary for testing them, (2) to review the history of the member companies prior to the merger in order to understand their relationship when they were independent, (3) to test the hypotheses by analysing the transactions between them following the merger, and (4) to consider another explanation of the behaviour.

Attention will be focused on two parts of the merger, the Dominion Coal Company and the primary steel mill. The steel plant was at one time separately incorporated as the Dominion Iron and Steel Company (Disco). In 1928 the merger was reorganized, and the newly created holding and operating company, Dosco, took over the operation of the steel plant; Disco disappeared as a corporate entity.

When Dominion Coal and Disco were joined in 1910, the preferred shares in each company were maintained. It is possible, therefore, that the two enterprises were administered as separate units. On the other hand, since the common shares in both were owned by a holding company it is also possible that they were merely parts of a firm. The general profit-maximization hypothesis must therefore be presented in two ways

[1] Cf. William Z. Ripley, *Main Street and Wall Street* (Boston, 1927), *passim*.

the unit for which profits were calculated may have been the individual enterprise, or it may have been the merger as a whole.

A. The Hypotheses

Transactions between companies may be of two sorts: either they involve commodities or services currently being produced, or they are of a financial nature, i.e. sales of securities, sales of capital equipment, and loans. Commodity transactions will be dealt with first.

1. Commodity Transactions

(*a*) **Individual Company Profit-Maximization.** Consider the implications of the assumption that Dominion Coal was an independent unit. It will be seen that Dominion Coal possessed monopoly power in relation to the steel plant. To test whether in this situation the coal company fully exploited its advantage would require an analysis of the steel plant's demand for coal. This is impossible since data on the marginal product schedule of coal in the steel plant are not available. However, we may reject the hypothesis that Dominion Coal was an independent unit, if, when a market was easily available, it sold to the steel plant at prices considerably below those it was receiving from other customers. Since the coal market was imperfectly competitive, the company might profitably have discriminated in favour of the steel plant when it could not readily find a market. The plant was a large consumer, it was isolated, and it would not resell to other buyers. A price might have been agreed upon which would have enabled Dominion to cover part of its sunk costs, when its production was low in relation to its capacity. Under conditions of low demand discrimination in favour of the steel plant would not indicate a merger-dominated policy, but at other times low prices to the steel plant would suggest that high profits for the coal company were not a primary objective.

It is not, however, a simple matter to determine when discrimination was taking place. Coal is not a homogeneous commodity, and the price data are in the form of annual averages for all sales. However, since steel production requires coal which cokes well and is relatively free of sulphur, it may be presumed that the steel plant took a high-quality coal and on this account would have been required to pay a price above the average of all sales. Thus if the steel plant was charged a lower-than-average price discrimination was being practised.

(*b*) **Total Merger Profit-Maximization.** Suppose now that the merger's profits were the major concern of the controlling group. The price entered on the books of the two divisions is irrelevant, since it would not

have affected the sum of the profits. The quantity of coal transferred was the important consideration. It would have been set at the point where the marginal revenue from the sale of coal to outside consumers was equated to the marginal revenue product of coal used in the steel plant. The determination of whether this quantity was actually arrived at is not possible with present data. But it is possible to show whether or not the profits of the merger would have been greater if *the steel plant had not been operated and if the coal which would otherwise have been used had been sold.* No assumptions regarding the shapes of the cost curves are required.

The following equations represent the gross profits of the coal company and of the steel plant in that order:

$$Y_c = PC + P_s C_s - O_c, \qquad Y_s = S - P_s C_s - O_s,$$

where Y_c stands for the gross profits of the coal company, P is the price of coal in the open market, C the quantity taken by the market, P_s the price paid by the steel plant, C_s the quantity taken by the steel plant, O_c the operating costs of the coal company, Y_s the gross profits of the steel plant, S the total revenue of the steel plant, O_s operating costs other than the cost of coal to the steel plant. The gross profits of the merger, the sum of Y_c and Y_s, may be represented as:

$$PC - O_c + S - O_s.$$

If the coal used by the steel plant were sold in the open market, provided that a market was available at the given price, gross profits would have been greater than those actually being earned when

$$PC + PC_s - O_c > PC - O_c + S - O_s$$

or

$$PC_s > S - O_s.$$

The term $S - O_s$ is the gross profits of the steel plant, Y_s, plus the cost of the coal to the steel plant, $P_s C_s$. I shall call $PC_s - P_s C_s$ the subsidy received by the steel plant. If the subsidy was greater than the gross profits of the steel plant, the merger would have earned more profits by not operating the steel plant.[1]

Long-run profit-maximization may have justified transfers of coal to the steel plant when short-run profits did not. The merger may have believed that the future of the steel plant warranted the continuation of its operations. The market for steel is imperfectly competitive, and it was perhaps advisable to maintain market contacts. Whether or not this was a factor

[1] It should be noted that when a marginal revenue product curve is linear and market for the raw material is competitive, the conclusion that a firm is not earning profits indicates that it should not operate at all. Professor T. O. Yntema found that marginal costs of steel production by United States Steel were constant over a very wide range of output. If we accept this finding the test suggested here cannot be considered extreme. Cf. United States Steel Corporation, *T.N.E.C. Papers* (New York, 1940), I, pp. 223–323.

will be apparent when the competitive position of the steel plant is discussed.

Finally, we must consider that what may appear to have been the result of a policy directed at some other objective than high profits may merely have been the product of mistaken anticipations. Firms, after all, do not always earn profits. However, gross profits are in question. Gross profits are total revenues less operating costs, and operating costs are for the most part variable. It is difficult to conceive of firms not knowing whether variable costs are being covered. If production is on contract and cost conditions change drastically from the date of a contract variable costs may not be met. But this is not likely to happen often. Thus if in several years the merger would have done better not to operate the steel plant, it would be almost certain that it was not directing its activities to high short-run profits.

2. Financial Transactions

When two companies are strictly independent there are few financial transactions between them. Loans of short duration may be made when the lending company has no pressing need of funds, or a company may purchase assets or securities of other enterprises when they represent good investments. Frequent financial transactions between two companies would indicate that they are not independent.

On the other hand, if the merger is the maximizing unit it would invest in the enterprise promising the higher rate of return. It might do so by having the strong enterprise sell securities to affiliated enterprises, by having it borrow from them, or by setting prices on intra-merger transfers so as to favour this enterprise.

Before the transactions within the merger are analysed a brief history of the two companies prior to their combination will be presented.

B. Before the Merger

In 1893 Dominion Coal was founded under the leadership of H. M. Whitney, a Boston financier. It purchased the major part of the Sydney field of Nova Scotia.[1] Flat seams and moderate depths kept costs low, and the demand for coal was large.[2] Profits were therefore high.[3]

[1] *Report of the Royal Commission Respecting the Coal Mines of Nova Scotia, 1925* (Halifax, 1926), p. 53. (Hereafter cited as Duncan Report.)

[2] David Schwartzman, *Mergers in the Nova Scotia Coalfields: A History of the Dominion Coal Company, 1893–1940* (Unpublished doctoral dissertation deposited with the University of California Library, 1953), Table III, p. 100.

[3] Eugene Forsey, *Economic and Social Aspects of the Nova Scotia Coal Industry*, McGill University Economic Studies No. 5 (Toronto: Macmillan, 1928), p. 11.

In 1899 Whitney and his associates organized the Dominion Iron and Steel Company. According to the promoters: the plant was to be built at Sydney, which was on tidewater; within a radius of twenty miles were large coal deposits; within two hundred miles by water were unlimited deposits of iron ore. However, they failed to mention that the ore was of inferior quality, that metallurgical coal was limited in quantity, and that the plant was far from its markets.[1] It is not surprising that Disco suffered losses. In spite of a railway construction boom and a favourably discriminatory price on coal, Disco was constantly threatened by a large floating debt.[2]

After 1902 Dominion Coal was no longer under the same control as Disco. Since other sources of coal of the quality required were distant from the plant and Dominion had alternative markets, Disco faced a monopolistic supplier. Although it was under contract to ship the quantities and the quality specified by Disco, Dominion never quite satisfied the demands of the steel plant, except in the winter when other markets were cut off. Frequently Disco was forced to accept slack and banked coal and to buy from other sources at high prices. In 1905 Disco took its problem to the courts, and in 1909 it was awarded damages of $3,500,000. At the same time the Privy Council released Dominion from its contract,[3] and Disco's coal supplies were more uncertain than ever.

To assure itself of sufficient coal at prices which it could pay Disco immediately took action to acquire control of the coal company. In November 1909 it purchased 50,000 shares.[4] Formal amalgamation was arranged through a holding company, Dominion Steel Corporation.

It was indicated earlier that a merger might be considered to be one or several firms from the standpoint of shareholders depending on its capital structure. Dominion Steel owned the controlling interest in Dominion and in Disco. This suggests that the interests of Dominion Steel shareholders were to dominate and that therefore the merger was to be operated as a single unit. However, an independent group of preferred shareholders was associated with Dominion and another with Disco, which implied independence for the operating companies. No inference can therefore be drawn from the capital structure as to the probable pattern of behaviour that the merger was to follow.

It may be concluded that had the two companies been merged earlier and if the merger maximized total profits it would probably have abandoned the steel plant. Despite a favourable market, the plant had not earned an adequate return.

[1] W. J. A. Donald, *The Canadian Iron and Steel Industry* (Boston and New York: Houghton, Mifflin, 1915), p. 201.
[2] Schwartzman, op. cit., p. 124.
[3] Dominion Iron and Steel Company, *Annual Report*, 1910.
[4] Ibid.

C. Intra-Merger Transactions

1. Dominion Steel Corporation 1910–20

Following 1910 the coal market continued to grow, but the demand for steel fell off with the cessation of the railway boom. During the war demand for steel was again high, but intensified competition was anticipated following the war. In spite of the history of losses and the poor prospects, the plant was renovated and enlarged. Financial aid was provided by the coal company. At the same time coal was supplied at prices well below the market and in quantities which were inconsistent with high profits to the merger.

The annual report for the fiscal year 1912 declared that sales of steel were still low. Nevertheless, in 1912 Disco added two blast furnaces, two open hearths, and new finishing mills. The market fell further in 1913.[1] During the war important additions were again made. A battery of coke ovens, a coal washery, new power stations, and a ship-plate mill were built, and older parts of the mill were remodelled. These investments were made despite the realization that following the war the steel market would contract and that there would be more favourably located large new plants.[2]

To finance the expansion of both Dominion and Disco, Dominion Steel in 1912 issued $7,000,000 in preferred shares. The revenue was to be provided out of an issue of $3,500,000 in bonds to the holding company by each of the subsidiaries. But at the end of the fiscal year 1912 the anticipated earnings of the additional steel capacity were not materializing, and the new obligations had weakened the corporation's current position.[3] To remedy this condition in 1913 Dominion Steel issued $3,400,000 in five-year notes secured by a mortgage on Disco's property and by its own guarantee. Disco's continued inability to sell at a profit prevented a substantial reduction in current liabilities in 1913, despite the note issue.[4] During the war the corporation reduced its debt, and in 1917 and 1918 generous dividends were paid on the common stock.[5]

Thus the two enterprises were not administered as independent financial units. The holding company's earnings before the war originated largely in Dominion Coal and enabled it to guarantee debts contracted by Disco. Moreover, rather than have Disco apply to the capital market directly, Dominion Steel issued preferred shares and bonds in its behalf. Investors were not required to base their decisions exclusively on Disco's potentialities.

[1] Dominion Steel Corporation, *Annual Report*, 1913.
[2] Ibid., 1917.
[3] Ibid., 1912.
[4] Ibid., 1913.
[5] Ibid., 1917, 1918.

It is also evident that the merger was not pursuing a high-profit policy in adding to the steel plant. Disco was only able to pay a fair return on its stocks during the war, and, indeed, it found it difficult to meet its funded obligations. Thus the hypothesis concerning the investment policy of the merger is inconsistent with the observations on the investments made in this period.

Turning to the transfers of coal, let us first see whether Dominion Coal was in this respect an independent unit. If it were, the price to Disco would have been at least equal to that received from other buyers, providing a market was available at that price. Throughout this period demand was high, and the company had no difficulty in disposing of its output. It should be recalled, moreover, that metallurgical coal commanded a premium over other coals.

Throughout this period the price charged to Disco was $1.55 per long ton.[1] In the table below the column headed 'Price obtainable', shows for 1913 to 1919 average prices received for coal produced by all Nova Scotia mines (not only Dominion Coal) exclusive of the coal consumed by the steel plant. These are estimates of the prices obtainable by Dominion in the market. Since Dominion produced a higher grade of coal than other Nova Scotia operators and was able to ship more easily to the central Canadian market, the estimates are probably too low. This is borne out by data on the prices received by Dominion in 1920, the only year for which such information is available, in relation to the average price at which Nova Scotia coal as a whole was sold. The range of prices paid to Dominion was $6.60 to $6.85, while the average for Nova Scotia was $6.32. The table also shows the quantity consumed by the steel plant and the subsidy received from the coal company. The subsidy is calculated as the difference between the price obtainable and the price paid by the steel plant multiplied by the quantity. It will be recalled that the subsidy referred to earlier was calculated in this way.

Dominion Coal would have earned higher profits by selling this coal to other buyers. It was not being administered independently. It is also apparent that the merger was transferring considerable amounts of funds to Disco; investment in the unprofitable subsidiary was thus further facilitated, and long-run profits to the merger as a whole were not being maximized.

Let us inquire whether the merger would have earned greater profits if it had closed the steel plant and had sold the coal which it consumed. If the profits of Disco were less than the subsidies shown above, the merger would have done better to have shut down the steel plant.

[1] *Official Record of Evidence Taken during Session of 1921 Respecting Future Fuel Supplies by a Special Committee of the House of Commons* (Ottawa, 1921), p. 134.

Subsidy to Disco from Dominion Coal 1913–20

	Price obtainable ($ per long ton)	*Quantity supplied* ('000 long tons)	*Subsidy* ($'000)
1913	2.72	1,213	1,395
1914	2.83	757	969
1915	2.73	1,072	1,265
1916	3.36	1,235	2,235
1917	3.96	1,235	2,976
1918	4.73	1,162	3,695
1919	4.90	881	2,951
1920	6.60	865	4,257

Sources: 'Price obtainable' in 1913 to 1919 is the average value per long ton of bituminous coal produced in Nova Scotia adjusted to exclude the consumption of the steel plant. Calculations were based on the Table of Quantity and Value of Production of Nova Scotia Mines in Dominion Bureau of Statistics, *Coal Statistics* (Ottawa, any year). 'Price obtainable' in 1920 is the minimum of the range of prices ($6.60 to $6.85) received by Dominion Coal in that year. Cf. *Official Record of Evidence Taken during Session of 1921 Respecting Future Fuel Supplies by a Special Committee of the House of Commons* (Ottawa, 1921), p. 148. 'Quantity supplied' from Dominion Steel and Coal Corporation, *Submission to Royal Commission on Coal 1944–45* (Sydney, 1945), p. 97.

In 1919 and 1920 the demand for steel was small, while that for coal was still large. In 1920 Disco's gross profits were about $2·5 million. The merger would have earned about $1·8 million more if it had not operated the steel plant.[1] In 1919 the gross profits of the corporation were $6,340,000.[2] For the merger to have earned profits on its steel operations, the gross profits of Disco would have had to exceed $2,951,000. In view of the state of the steel market, the high price of coal, and Disco's competitive position, it is almost certain that Disco's profits were less than this figure.

Thus the merger in 1919 and 1920 was apparently not primarily interested in high short-run profits, and throughout this period it does not appear to have been concerned with high long-run profits.

2. The British Empire Steel Corporation 1921–8

In 1921 Dominion Steel combined with Nova Scotia Steel and Coal (Scotia) and Halifax Shipyards under a new holding company, the British

[1] Estimate of profits of Dominion Steel made by splitting profits reported in fiscal years 1919 and 1920. Cf. Dominion Steel Corporation, *Annual Report*, 1919, 1920. Profits of Dominion Coal estimated on basis of data supplied in Duncan Report (op. cit., pp. 30–31), which stated that in 1920 Dominion Coal earned 13·2 per cent. of the book value of its assets. For the value of its assets I used the value reported in British Empire Steel Corporation, *Annual Report*, 1927. Profits of Disco were estimated as the difference between those of Dominion Steel and those of Dominion Coal.

[2] Estimated by splitting gross profits for fiscal years 1918 and 1919. Cf. Dominion Steel Corporation, *Annual Report*, 1918, 1919.

Empire Steel Corporation (Besco). Besco exchanged its own shares for the common of the constituent companies. Otherwise their capital structure was undisturbed.[1] The merger remained a single unit from the standpoint of common shareholders, while the interests of preferred shareholders were still distinct.

Coal prices continued to climb until 1923 when a slight fall occurred. Steel prices, however, did not revive before the latter half of the twenties. Nevertheless, in 1921 Disco installed additional coking capacity and new repair shops. For this purpose and to reduce current liabilities, Disco issued $4,645,000 in mortgage bonds supported by a guarantee of Dominion Steel.[2] The additions were not effective in raising Disco's profits, and beginning in 1924 strenuous efforts were made to reduce expenditure.[3] But Disco was unable in 1926 to meet its obligations to holders of first mortgage bonds, and it was placed under a receiver.[4]

Since Dominion Steel had guaranteed the bonds, the receiver applied for its dissolution. In the course of the conflict between the interests concerned, a new group acquired control of the merger. Holt, Gundy, and Company, a Toronto investment house, purchased over 90 per cent. of the defaulted bonds and by this action was able to gain control not only of Disco and Dominion Steel but of Besco as a whole.[5] In 1928 the Dominion Steel and Coal Corporation (Dosco) was organized to take over Besco's assets. The new corporation issued new first mortgage bonds in exchange for Disco bonds; Disco thus lost its identity. At the same time the intermediate holding company, Dominion Steel, was dissolved.[6]

Thus even as late as 1921 Dominion Steel guaranteed a bond issue by Disco, permitting it to enlarge its plant further. The corporation again did not adhere to an investment policy consistent with high long-run profits.

Moreover, up to at least 1924 the intra-merger price on coal remained at $1.55 per long ton. The evidence on the intra-merger price in 1925 is conflicting. In 1926 an agreement between the receiver for Disco and Dominion Coal set the price at the level available in Montreal less transportation.[7] So long as Disco was under the control of Besco, except

[1] *Memoranda Relative to the Constitution of the British Empire Steel Corporation* (Montreal, 1927), p. 22.

[2] Dominion Steel Corporation, *Annual Report*, 1921.

[3] *Financial Post*, 15 August 1924, p. 15.

[4] British Empire Steel Corporation, *Annual Report*, 1926.

[5] Ibid., 1927.

[6] Dominion Steel and Coal Corporation, *Annual Report*, 1930.

[7] *The Report of the Royal Commission on Coal* (Ottawa, 1946), p. 128, is vague about the date at which the subsidy was terminated. It states that by 1925 the price was $3.13 for slack and $3.82 for run-of-mine, which were the same as the prices agreed upon in 1926, but no relationship is drawn between the two sets of prices. The Duncan Report of 1926 stated that 'the operators claim that the wage problem of the miners must be looked at in the light

possibly in the last year, the price of $1.55 was maintained. Dominion Coal was not independent, and the merger continued to channel funds into the uneconomic enterprise.

The funds transferred to Disco by means of the low price are shown in the table below.

Subsidy to Disco and its Gross Profits 1922–4[1]

	Price obtainable ($ per long ton)	*Quantity supplied* ('000 long tons)	*Subsidy* ($'000)	*Gross profits* ($'000)
1922	5.36	535	2,039	300
1923	5·43	981	3,806	3,000
1924	4.74	735	2,344	500

Sources: See previous table for price and quantity data. Profits are estimated by residual in the same manner as described in n. 1, p. 205. The Duncan Report's estimates of the rate of return on Dominion Coal's assets were used together with the book value of assets in 1927.

The table also compares the subsidy with the gross profits earned by Disco. It shows that the merger as a whole would have earned higher profits by not operating its steel plant and by selling the coal.

It may be concluded that in this period the merger did not attempt to maximize either its short-run or long-run profits.

3. The Dominion Steel and Coal Corporation 1930–9

The new holding company acquired all of the common shares in Dominion Coal and became the operator of the steel plant. The preferred shares in Dominion Coal were the only publicly held shares apart from the common shares in Dosco. Again, if decisions were based on the interests of common shareholders Dominion Coal would have been operated as an integral part of the merger, but if those of the preferred shareholders were primary it would have been allowed to operate independently.

Both the coal and steel markets were depressed, and the competitive position of the steel plant continued to deteriorate. The market for steel had shifted from rails to structural shapes and light steels, but the plant

of a consolidated undertaking'. It made a special point of urging the corporation to enter intra-corporate transfers at market prices for the purpose of setting wages (op. cit., pp. 12–13). Moreover, the Besco Annual Report for 1925 urges that the corporation should be regarded as a unit.

[1] Since Besco included Scotia and the Shipyards as well as the companies with which we are principally concerned these estimates of profits are in error. The error, however, is to overestimate Disco's profits and does not, therefore, prevent the test of the hypothesis. The Scotia steel plant was closed in 1921, so between 1922 and 1924 no operating losses originated there, and the Scotia coal-mines were earning profits. Earnings of the Shipyards are unknown, but they were not sufficiently important to affect the estimate significantly.

was still essentially a rail mill. Moreover, technological changes in primary steel production had reduced the importance of coal and ore as locational attractions. Improvements in the utilization of coal and increasing use of scrap rather than ore were drawing steel plants towards market sites.[1] In addition, the increasing diversity of steel products reinforced the attractiveness of such locations. It was difficult to keep a great variety of shapes and sizes in stock, but with the plant in a market area quick deliveries could be offered despite small stocks. While Dominion Coal had no difficulty in meeting its obligations, Dosco was not so fortunate.

In 1932 Dosco was unable to continue interest or sinking fund payments due on its first mortgage bonds and was heavily indebted to its bankers. The settlement of claims included an issue of debentures by Dominion Coal which cancelled the income bonds held by the corporation.[2] Dosco thus replaced some of its own bonds outstanding with those of Dominion.

Although profit expectations were still low, in 1937 Dosco embarked on a programme of expansion. It purchased the capital stock of the subsidiaries of United States Steel located at Walkerville, Ontario, and of the Graham Nail and Wire Company of Toronto. In 1939 Sarnia Fence in Sarnia, Ontario, was acquired. The object of these acquisitions was to secure control of outlets for the steel plant's output of ingots.[3] In addition, the Sydney plant was remodelled. New boilers, an electric furnace, three mechanical gas-producers, two soaking-pit furnaces, a heavy wire-drawing block, and two continuous wire-drawing machines were installed. The rod mill was electrified and the blast-furnace stoves were renewed. Ingot capacity was raised from 425,000 to 600,000 tons.[4]

Dosco obtained financial assistance for these investments from the coal company. Dominions purchased from Dosco the common stock of the Cumberland Railway and Coal Company for $1,000,000,[5] and the common stock of the Dominion Rolling Stock Company for an undisclosed sum.[6] Although it cannot be said that the prices on the assets transferred were high, it is evident that the corporation shifted funds from Dominion to itself in order to facilitate the purchase of the Ontario plants and the extension of the primary plant. This policy was adopted despite the earnings record of the primary plant and its unsatisfactory competitive position.

Turning again to intra-merger transfers of coal, Dominion Coal stated that between 1933 and 1936 prices to the steel plant were 45 cents below

[1] Walter Isard and William M. Capron, 'The Future Locational Pattern of Iron and Steel Production in the United States', *Journal of Political Economy*, Apr. 1949, lvii, pp. 118–33.
[2] Dominion Steel and Coal Corporation, *Annual Report*, 1934.
[3] Ibid., 1937, 1939. [4] Ibid., 1937, 1939, 1940.
[5] Report of the Royal Commission on Coal, op. cit., p. 134.
[6] Dominion Steel and Coal Corporation, *Annual Report*, 1937.

those received for the same grades in Montreal and that in 1939 and 1940 the price differential was 50 cents. According to the company:

> In the years 1933, 1934, 1935 and 1936 when business was greatly depressed, the Steel Company endeavoured to (and did) obtain considerable export business. To obtain this business it was necessary for the Steel Company to reduce the selling price of its product considerably below the domestic price. An increased production of the Steel Company meant increased coal consumption, and the directors agreed to reduce the price of coal supplied and used in the production of steel for export in the amount of forty-five cents per gross ton in the years 1933 to 1936 inclusive, and in the years 1939 and 1940 in the amount of fifty cents per gross ton.[1]

In imperfectly competitive markets when demand is low in relation to total capacity rather large price cuts may be necessary before a producer can achieve a high level of production. In such periods profits may be increased by a discriminatory price policy. In this instance so long as consumption by the steel plant was not replacing sales to other customers willing to pay higher prices, a lower price to Dosco than the average received from other buyers might have been profitable, providing it was not below average variable costs. The transfers to Dosco were probably not replacing sales to other customers. At no time in this period did Dominion operate above 85 per cent. of capacity, and in the years in which the price differential was granted the highest was 81 per cent. of capacity.[2] It is unlikely that other customers paying higher prices could have been found for the volume taken by the steel plant. Further, the price to the steel plant was not below average variable cost. In 1936 Dosco was charged $3.63 per long ton of run-of-mine coal, and Dominion's average total cost was $3.49. In 1939 the amounts were respectively $4.00 and $3.82.[3] Not only were the prices above average variable costs but they even allowed Dominion Coal a small profit.

Since it is possible that this policy might have been followed by an independent coal company, it is also possible that the merger was maximizing total short-run profits. The merger may have been doing as well as it could under the circumstances.

But by means of a low price and through the sale of assets the merger financed the enlargement of the steel plant. Its control of the coal company permitted it further to develop a property which had long been a source of loss and whose competitive position had further deteriorated.

Thus in all of the three periods the merger's behaviour was not consistent with the maximization of long-run profits either to Dominion Coal or to the merger as a whole, and in the two earlier periods there were several

[1] Report of the Royal Commission on Coal, op. cit., pp. 128–9.

[2] Dominion Steel and Coal Corporation, *Submission to Royal Commission on Coal 1944–45* (Sydney, 1945), p. 93.

[3] *Evidence Presented before the Royal Commission on Coal* (Mimeographed, 1945), p. 3980; Report of the Royal Commission on Coal, op. cit., p. 129.

years in which its behaviour does not appear to have been aimed at high short-run profits.

D. An Alternative Explanation

A theory based on profit-maximization as the guiding motive does not provide a satisfactory analysis of the behaviour studied. A better explanation of the merger's policies would focus on its fear of bankruptcy and its capital structure. If a firm comprises only a single company, a policy aimed at minimizing risks of large losses would not lead to behaviour which is inconsistent with high profits. But where the firm is a merger of companies, each with its own debt and body of shareholders, profit-maximization may conflict with the interests of the controlling group. It may then be important for this group that the output of an enterprise be maintained and that its equipment be improved in order to preserve its own equity in the merger, even though total profits are reduced by the protective policy.

When a dissolution occurs, the assets of the company may be sold, the capital structure may be reorganized, and control may be assumed by a new group. To satisfy creditors the assets may be disposed of at a fraction of their value as part of a going concern. When this is impossible, the company is usually reorganized and the values of junior securities are reduced in relation to other securities. At the same time, bondholders may acquire control of the firm and replace its management. A dissolution is, therefore, to be feared by owners of junior securities and by management. Whenever their interests are primary, any course of action which threatens a firm's liquidity is likely to be rejected despite the equal possibility of high profits.

When a firm comprises only one company a policy of minimizing risks of losses would not differ significantly from one directed at maximum profits. Professor Fellner suggests that risks of losses are minimized at an output that maximizes the difference between average revenue and average variable cost, or, as he calls it, 'the safety margin'.[1] Since, however, the precise shapes of the curves are not known in advance, the alternative objectives do not lead to significantly different outputs. Nor will a policy directed at minimizing risks of losses lead to investment behaviour highly inconsistent with that prescribed by high profits. The firm may invest less capital, but it will not invest in its own activities if there is little prospect of a return at least equal to those being earned in other firms.

When, however, several companies are closely linked but not wholly owned by a holding company, it may be rational for certain groups of

[1] William Fellner, *Competition Among the Few* (New York, 1949), pp. 146–57.

shareholders to ensure that one of the companies be subsidized. The fact of separate rights of groups of shareholders and bondholders associated with each of the plants within the merger is the important feature which distinguishes this type of corporate structure from that of an independent enterprise.

The problems of the merger revolved around the large debt acquired in the construction, growth, and operation of the steel plant. At the moment that sinking fund and interest payments ceased, the principal fell due. Since it was the guarantor, Dominion Steel was called upon to pay the principal or be dissolved. It was in no position to discharge its obligation. Moreover, the holding company might have been required to pay off its own debts before it paid those of its subsidiary. Its control of Dominion Coal permitted it to avoid dissolution by requiring the coal company to subsidize the steel plant.

The question arises whether it would not have been possible for Dominion Coal to have paid to Dominion Steel sufficient dividends for the latter to have paid the interest and sinking fund charges on Disco's debt. This would have permitted the closure of the steel plant and a larger aggregate profit for the merger. There are two objections to this procedure.

First, dividends on Dominion Coal's common could only be paid after adequate provision was made for depreciation and after dividends on the preferred. Whatever the long-run advantages of the procedure would have been, it would probably not have been possible in the short run. Profits from coal operations in any one year were uncertain, and dividends might not have been sufficiently regular or large enough to meet bondholders' requirements.

Second, for Dominion Steel to have openly paid the interest on Disco's debts and to have closed the plant would have been an admission of failure, and it would have hurt the holding company's credit even more than the concealed losses of the steel plant.

Alternatively, Dominion Coal might have been consolidated with Dominion Steel. Dominion Steel might have purchased the preferred shares and bonds in Dominion Coal, or Dominion Coal might itself have done so. The two companies would thus have been combined, since Dominion Steel was the only common shareholder in Dominion Coal. The interest and sinking fund payments on Disco's debt could then have been paid directly out of Dominion Coal's profits. Six million dollars was required for the purchases, however, and it was never available at any one time. A fund might have been built up for this purpose, but in the meantime Disco required support.

To protect itself against dissolution the holding company, therefore, had the coal company transfer funds to the steel plant by means of a low price

for coal, and production was maintained. Moreover, if the steel plant was to continue operating it had to be made as profitable as possible, so that whatever funds were raised were invested in the plant.

The position of common shareholders in Dosco was similar to that of shareholders in Dominion Steel. They were directly responsible for the payments on a large debt. They were unable to meet the payments due and the corporation was threatened by dissolution. They could not acquire the assets of the coal company, nor could dividends on its common be assured after depreciation and preferred dividends. Therefore, funds were channelled into improvements and the enlargement of the steel plant in the hope of raising its earning power.

Thus it is apparent that the property rights associated with Dominion Coal prevented the merger from acting as a profit-maximizing unit. A single-company firm would have been able to avoid operating the unprofitable steel plant. But it was not to the interest of the holding company to abandon the plant so long as it could not freely distribute the earnings of the coal company.

This behaviour was the result of a complicated capital structure. Although the structure of Dosco and more particularly of Besco was unusually complex, the elements which led to the policy of maintaining the steel plant were not exceptional. For similar policies to be anticipated in other mergers, it is required (1) that there be two companies, each with its own bonds and shareholders, (2) that one of the companies be threatened with dissolution, (3) that this company have control of the other, and (4) that it is not financially possible to amalgamate the two companies completely. The result may be that a plant which would otherwise be abandoned owing to high costs of operation would continue producing.

E. Conclusion

While in the case of Dosco a condition of primary importance was the threat of dissolution, a policy of favouring one enterprise at the expense of affiliates may be followed without this inducement. The presence of separate shareholder rights may be sufficient. Simply because the dominant interest is associated with it, a company which is part of a merger may be favoured. Transfers of goods or services between companies may be made at prices below the market. Sales of assets, securities, leases, or other properties may be made on terms designed to assist the growth of one company. A single-company firm, on the other hand, would not have any inducement to promote one of its activities at the cost of others unless it promised a higher profit. It is therefore more likely to act in a manner consistent with welfare requirements.

The interests of minority shareholders are protected by law, and the threat of legal action may restrain officers from grossly exploiting such interests. However, in general, acts of officers are only questioned at the instigation of injured parties. Information on the relationship of companies within a merger is generally not available, and no case can therefore be brought to the attention of the courts. In the case of the merger studied here, information on intra-merger prices only became available as a result of special inquiries undertaken by the House of Commons, and the terms of the sale of Cumberland Railway stock were revealed by a Royal Commission.

An attempt has been made to protect investors against officers of holding companies in the public utility field in the United States. Under the Public Utilities Holding Company Act of 1935, the Securities Exchange Commission was empowered to supervise the relationships between public utility holding companies and their subsidiaries in order to ensure that no investors be unfairly treated. The S.E.C. receives reports of payments between companies under any one holding company. Control has been detailed, persistent, and effective in the defence of shareholders.

As might be expected of any judicial or quasi-judicial body, the Commission has stressed considerations of equity. Its effectiveness is therefore limited as a means of achieving desirable economic ends. Such bodies cannot be depended on to require that a multiple-company merger be administered so that the guiding motive is maximization of the merger's profits, nor can they ensure that each enterprise is independently operated. In our particular case, the transfers of assets and securities which enabled the growth of the steel plant could not have been challenged at law. There was no inequity in the prices set. Moreover, when goods are transferred within a merger, it may not always be clear whether the price at which the transfers are made are unjust. Only when a market is fairly stable, when the commodity in question is of standard quality, when there is a large volume of transactions, and when discrimination is not practised in dealings between independent firms in that market, can a clear case be made. Further, assistance may be given by one company to another without any transaction being recorded in the books of either. Personnel and equipment may be 'loaned' without there being any documentary evidence of such assistance. The protection available to shareholders cannot, therefore, be counted on to ensure behaviour consistent with general economic welfare.

The only alternative would appear to be the enforced simplification of corporate structures. Intercorporate stock ownership may be abolished if its object is to control other companies without owning them completely. Specifically, companies might be permitted to own either less than 1 per cent. of the shares of another company or not less than 100 per cent. It

may not be possible to set a statutory percentage which a company could hold of another's stock: in some instances even 1 per cent. may be sufficient to acquire control, and in others a 1 per cent. limit may be far below the minimum necessary for control and at the same time unduly restrict the investment opportunities open to corporations. It may be necessary to give authority to an administrative agency to set the percentage allowed. Thus intercorporate stock ownership would be permitted when the purpose is investment, or when a firm wishes to avail itself of the protection offered by separate incorporation of the various enterprises which it owns. The measure would prohibit purchases of shares for the purpose of obtaining control without acquiring complete ownership.

It may be protested that a holding company is not necessary for a group of persons to have control of several companies. But it is rendered much more difficult if the instrument of a holding company is not available. The group would have to be closely knit, since it could not easily delegate authority, and large resources would have to be at the personal disposal of its members. It could not make use of outside capital. While not all avenues to the control by a close group of multiple-company systems would be closed, the effect of the measure would be to reduce the importance of such combinations substantially.

Further, it may be objected that some desirable mergers would be rendered costly and they would therefore be blocked. Economies of scale of firm would be sacrificed. The evidence on the importance of this type of economies is still uncertain. Moreover, where they are important the measure proposed here should not be a serious obstacle. Companies will still be able to combine after negotiation and agreement on the relative values of their respective assets.

Partial ownership of one company by another is a common phenomenon. Where it occurs motives other than the maximization of the profits of the combination may dominate, and one company may not be allowed to operate without regard to the interests of the other. It is likely that in such cases the controlling group will favour one of the companies. High costs may then be tolerated, and plants may be expanded without close regard to their relative profit possibilities. The proposed public policy on multiple-company mergers would prevent such behaviour at no great cost.

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THE LONG-RUN DOLLAR PROBLEM: A COMMENT

By E. J. MISHAN

WITH his customary deftness in the application of economic theory to some current issue, Professor Hicks[1] has stripped the problem of all inessentials and nicely selected the few tools necessary to get at once to the heart of the matter. His treatment, which leaves us with such notions as export-biased and import-biased goods, has in fact provided the proper empty boxes for the empiricist whose task it is to fill them and derive therefrom the appropriate conclusions. To some extent, albeit in a casual fashion, Professor Hicks also assumes the role of empiricist, and no doubts are cast on the suggestive historical remarks or on his guesses about current trends if it is observed that in one or two particulars the theoretical treatment appears more categorical than is perhaps warranted.

Let us recall the summary of his theoretical conclusions: (i) if 'American' technological advance is spread uniformly over the whole economy, the terms of trade are likely to be favourable to 'Britain'. (ii) If the technological advance is concentrated upon the goods which America exports, then the terms of trade will almost certainly favour Britain, while (iii) if it is concentrated upon those American goods which compete with American imports the terms of trade will move against Britain.

Now such inferences may be satisfactory enough if the income effects involved in the analysis are slight relative to the substitution effects. If, however, we can as yet say nothing of the relative magnitude of these effects it may be shown, (*a*) by removing the assumption of constant costs (a procedure which is, admittedly, open to dispute), or (*b*) by removing the static assumptions of the model (a procedure which is hardly open to dispute), that either a uniformly spread or an export-biased technological advance in the U.S. does not of itself ensure that the terms of trade move in favour of the U.K. This means that if the empiricist wishes to make any pronouncement concerning the current trend of the terms of trade inasmuch as they are affected by technology he will not only have to discover the general bias of American technological innovation but he will have to unearth data on the relevant income effects as well.

Before commenting on the productivity analysis proper, it might be useful to point out that neither the commodity terms of trade nor the factorial

[1] J. R. Hicks, 'The Long-Run Dollar Problem', *Oxford Economic Papers* (New Series), vol. v, No. 2, June 1953.

terms of trade,[1] with which Professor Hicks illustrates his conclusions, are in principle adequate as an index of the welfare of a country arising from its international transactions. For example, a reduction in the cost of British exports, import prices falling by a smaller percentage, entails an adverse movement for Britain of the commodity terms of trade. Again, a rise in American money wage rates accompanied by some decline in the prices of those American goods imported into Britain entails, *cet. par.*, an adverse movement for this country of the factorial terms of trade. Either eventuality, however, plainly advances the welfare of Britain, a result which is revealed in each case by the employment of appropriate 'welfare' terms of trade—say, an index of average money wage rates, or average money incomes, divided by an index of the price of imports.[2] Nevertheless, it so happens that for the 'passive' country—the country in which we suppose no change in productivity to take place and on which we are to focus our attention (Britain, in Professor Hicks's illustration)—the changes in welfare arising from the alternative sorts of innovation we presume to take place in the other country (the U.S.) are as adequately represented by the commodity terms of trade as by the welfare terms of trade suggested above:[3] an improvement in the commodity terms of trade for the passive area turns out to be an improvement in the same degree if measured by these welfare terms of trade, and is accompanied also by an expansion in the volume of trade. The converse is, of course, true for an adverse movement of the commodity terms of trade. We are therefore quite justified in this analysis in considering, as in fact Professor Hicks does, the commodity terms of trade of the passive area—the U.K. in his illustration—as a suitable index of the change in welfare arising from its trade with the 'active' area, the U.S.

[1] The factorial terms of trade as described by Professor Hicks resemble Viner's 'double factoral' terms of trade—an index of the real change in country *A*'s wage rates divided by an index of the real change in country *B*'s wage rates. See Jacob Viner, *Studies in the Theory of International Trade*, p. 560.

[2] These 'welfare' terms of trade give roughly the same effect as Viner's 'single factoral' terms of trade which he derives from multiplying the commodity terms of trade by the 'reciprocal of the index of cost in terms of the quantities of the factors of production used per unit of export'. The resemblance follows since, using the Viner method, dividing the numerator of the commodity terms of trade—the index of export prices—by Viner's index of labour cost per unit of export converts it to an index of money wage rates in export industries, and this is not very different from the index of average money wage rates in general, the numerator of my 'welfare' terms of trade. See Jacob Viner, *Studies in the Theory of International Trade*, p. 559.

[3] Since, by assumption, there is no technological progress in the 'passive' country, the index of money wage rates and the index of export prices in that country will move together if they move at all. But these indices are the numerators, respectively, of the welfare or 'single factoral' and the commodity terms of trade. As for their denominators, they are both the same—an index of import prices. Consequently these two different terms of trade indicators will fall, or rise, together in response to a rise, or fall, respectively, of their denominators, or a fall or rise, respectively, of their numerators.

Turning now to the productivity analysis, let us reconsider Professor Hicks's case (i). The gist of his argument may be summarized as follows: given constant costs everywhere, an increase in American productivity spread uniformly over the whole economy will raise money wages and/or lower prices of the whole range of American goods by the same proportion. Since the *relative* prices of all American goods remain exactly as before then, provided aggregate real income in the two countries remains unchanged, the rate of exchange which restores equilibrium between them[1] will also restore the identical volume and pattern of trade in existence before the technological advance, and therefore leave the terms of trade unchanged as between the two situations. However, once we take account of the fact that an increase of American productivity entails an increase in American real income which may legitimately be expected to involve an increase in the U.S. demand for British manufactures, the terms of trade move in favour of Britain. When we lift the assumption of constant costs[2] and insert in its stead the assumption of increasing cost, this conclusion no longer holds. The demonstration follows at once.

Suppose the U.S., producing both grain and textiles, exports grain to the U.K. and in exchange imports textiles. Now in order merely to maintain the rate of exchange (and, therefore, in this model, the terms of trade) the U.S., after her productivity has been increased in both commodities, must want to continue to exchange, on those terms, the same amount of grain for textiles. But, to *exchange* no more than the same amounts as before on the same terms, requires that the U.S. *consume* an additional quantity of textiles. For, with an all round increase of productivity, she produces, at the *same* relative prices as before, more of both textiles and grain.[3]

Hence, for the terms of trade to move in favour of the U.K., not only is a positive income effect on textiles necessary but—since the U.S. itself now produces more textiles—the positive income effect must be above a determinate magnitude. If the income effect on textiles in the U.S. is smaller than this magnitude the terms of trade will move against the U.K.

[1] If we wish to eliminate consideration of the monetary problem, we can ignore the level of prices and wages in the two countries. Assuming a perfectly flexible exchange rate, we concern ourselves directly with the terms of trade resulting from the movements in the rate of exchange.

[2] With import-competing goods in the two countries, the two-country two-commodity model impels a consideration of increasing cost (unless we allow for a constant cost country being so large that it gains nothing from international trade). However, Professor Hicks can maintain his constant cost postulate (or even a diminishing cost postulate for that matter) along with the prevalence of import-competing goods if there are four, or more, commodities in the model—that is, if each country, though it does not produce the identical goods it imports does produce similar goods, goods which are highly substitutible in consumption.

[3] This last proposition is the consequence of the increasing cost postulate. With constant costs she can produce *any* combination of goods given by the production frontier at the same relative prices as before.

The argument can be elaborated with the aid of two conventional diagrams. In Fig. 1 offer curves for the U.S. and the U.K. are depicted, BQ representing the equilibrium quantities exchanged between them at the terms of trade given by BT. If at these terms of trade the U.S., after her technological improvements, wished to exchange more grain for U.K. textiles (so that her new offer curve cut the pre-existing terms of trade line,

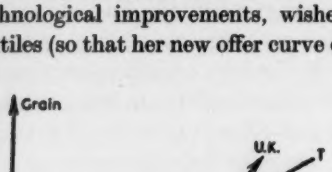

Fig. 1.

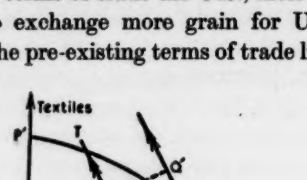

Fig. 2.

BT, to the right of Q) then the terms of trade would turn against her and in favour of the U.K. On the other hand, if after these improvements she wished to exchange less grain for U.K. textiles, the terms of trade would move against the U.K. and in her favour.

Fig. 2 depicts the U.S. production possibility curve, PP, before technological innovations take place. The terms of trade are given by BT. Total production in the U.S. is represented by the point B, total consumption by the point Q, with BQ—equivalent to BQ in Fig. 1—representing the quantity exchanged with the U.K.

An overall advance in U.S. productivity is represented by the curve $P'P'$, drawn so that tangents to the two production possibility curves—such as B and B'—are parallel where they are cut by a straight line passing through the origin. Now, in order for the same terms of trade to prevail as preceded the innovations, the combination produced must be at B' and the combination consumed at Q', where QQ' is drawn parallel to BB'. The actual income effect required is the vertical distance between QS and Q', which may conveniently be suggested by SQ'. If the U.S. income effect on textiles is smaller than that indicated by SQ' then the quantities she offers for exchange at the going terms of trade will be smaller than $B'Q'$ and therefore the resultant terms of trade will move in favour of the U.S. and against the U.K. Conversely, if the U.S. income effect on textiles is larger than that indicated by $Q'S$ the amounts offered for exchange by the U.S. are in excess of those represented by $B'Q$ and the terms of trade turn in favour of the U.K. and against the U.S.

Furthermore, even if the U.S. has a technological advance which is concentrated solely in grain production so that the new production possibility curve is to be drawn from P on the Y-axis to P' on the X-axis, provided only that a point B' on this new production possibility curve (whose tangent is parallel to that of B) is higher than B, some determinate positive income effect in favour of textiles will be necessary if the terms of trade are to move in favour of the U.K.

Finally, if we look beyond the static model then even a U.S. marginal propensity to consume textiles above the crucial values in these two cases will not necessarily move the terms of trade in favour of a U.K. whose real income is also increasing, albeit at a slower rate than that of the U.S. Nor, indeed, will the resumption of the postulate of constant cost, in these two cases, ensure that the terms of trade move in favour of the U.K. For the U.K.'s marginal propensity to consume grain may be much higher than the U.S. marginal propensity to consume textiles and, therefore, notwithstanding her slower technological advance, the U.K.'s increased demand for U.S. grain may exceed the increased demand of the U.S. for British textiles.

Moreover, once we admit U.K. technological improvements into the model, the three categories of innovation—uniform, export-biased, and import-biased—apply also to that country. As between the two countries then nine combinations are possible,[1] each combination, with given marginal propensities in the two countries, has a place in the order of possible effects on the terms of trade. The most unfavourable combination for the U.K. would be that of export-biased innovations in that country coupled with import-biased innovations in the U.S.[2] This situation would be aggravated, from the point of view of the U.K., by a high marginal propensity to consume grain and a low marginal propensity of the U.S. to consume textiles. At the other extreme would be the most favourable combination for the U.K. with import-biased innovations in that country accompanied by export-biased innovations in the U.S., a situation which would be enhanced by a low U.K. marginal propensity to consume grain and a high U.S. marginal propensity to consume textiles.

In conclusion we might ponder briefly two ideas raised by the Hicks analysis: (1) the meaning of a growing technological differential between

[1] Professor Hicks could, of course, have confined himself to just two categories, import-biased and export-biased innovations, with innovations spread uniformly over the economy as something mid-way between the extremes. But there is an obvious pedagogical advantage in considering, at the start, the case of a uniform technological advance.

[2] To illustrate, the quantity of textiles offered by the U.K. in exchange for grain when textiles alone are produced more abundantly will be larger than it would be if instead innovations made grain, or both grain and textiles, more abundant. While in the U.S., the amount of grain offered in exchange when textiles alone become more abundant will be smaller than it would be if American innovations made grain, or both grain and textiles, more abundant.

the U.S. and the U.K. or, more broadly, between the U.S. and Western Europe. In appraising the apparently more advanced methods of production in general use in the U.S. we have always to distinguish between (*a*) those methods of production which are the consequence either of the relative abundance of capital or natural resources or the greater size of the market in the U.S., which methods would prevail there even if technological advance were the same in all the countries concerned, and (*b*) those methods of production in the U.S. which, with existing factor prices in Western Europe, might be profitably adopted there. Strictly speaking, it is the existence only of the latter category which spells technological inferiority. And, in the absence of effective U.S. monopoly, this inferiority must be attributed to lack of knowledge or lack of enterprise. Not all economists would concede as much. But among those who would, how many, I wonder, expect the technological lag—if there be one—not only to continue but to grow over time.

(2) As for the current trend toward innovations being concentrated in the import-competing goods sectors of other countries, about which Professor Hicks speculates, how much of this phenomenon can properly qualify as technological innovation? Surely much of it is the result rather of deliberate policies which aim to diversify, behind new trade barriers, the economies of certain countries hitherto specialized to a high degree and exposed to the inevitable vicissitudes of international commerce.[1]

[1] Nonetheless, such activity, for instance attempts to industrialize the raw material producing areas, will clearly tend to move the terms of trade against the industrial economies of Western Europe.

'ECONOMIC ASPECTS OF COMPULSORY TRADE UNIONISM': A NOTE

By MONROE BERKOWITZ

MR. V. L. ALLEN, in the February 1954 issue of this journal,[1] presented a thesis to explain managerial attitudes towards compulsory trade unionism and examine why this phenomenon is more widely accepted by employers in the U.S.A. than by managers in British industry. It is the purpose of this note to raise questions about the applicability of Mr. Allen's ideas to the current industrial arrangements in the U.S.A.

According to Mr. Allen, an employer's attitude toward compulsory trade unionism will depend upon his 'concession curve' which is derived from the relative costs of concession and resistance. Concession costs are measured by the effect of compulsory unionism on the efficiency of labour in the enterprise while resistance costs are the price of not granting this demand and possibly being faced by a work stoppage.

Mr. Allen reasons that since labour in Great Britain has been relatively cheap as compared with capital, the resulting extensive methods of production have made it incumbent upon employers to guard jealously their control over the utilization of labour. At the same time, the low overhead costs relative to labour costs tend to make work stoppages less disastrous. Consequently, the British employers, faced with high concession costs and low resistance costs, have refused to look with favour upon compulsory membership provisions. In the U.S.A., on the other hand, these factors are said to operate in reverse. Expensive labour has led to highly mechanized, intensive production methods under which employer control over the work force becomes secondary to the necessity of maintaining continued production. Hence, it is reasoned that, in the United States, employers' concession costs are low and their resistance costs high. It is essentially these differences in the nature of the productive processes which explain, in Mr. Allen's view, the endemic nature of compulsory trade unionism in the U.S.A. as compared with Great Britain.

In considering the validity of Mr. Allen's thesis as applied to the U.S.A., the matter of concession costs will be examined first. Under compulsory trade unionism, the employer is said to lose 'the facility to select workers according to their ability and to regulate their engagement with the firm'.[2] It is doubtful whether this is an actual consequence of compulsory mem-

[1] 'Some Economic Aspects of Compulsory Trade Unionism', vol. vi, No. 1, pp. 69–81.
[2] Ibid., p. 73.

bership provisions in the U.S.A. at the present time for a number of reasons.

1. Interference with an employer's ability to select workers at the time of their *original* hire can be affected only by a closed shop arrangement under which workers must be members of the union before being hired.[1] The closed shop is banned under the Taft-Hartley Act, the current labour relations code in the U.S.A.[2] Even before the effective date of this 1947 statute, however, the closed shop was not popular in those industries which Mr. Allen postulates as characteristic of the U.S.A. Unions in the highly mechanized, mass-production industries, such as steel, automobiles, aluminium, &c., never pressed for the closed shop, possibly because of their inability to undertake the personnel functions involved in hiring for such huge aggregates. Quite contrary to Mr. Allen's thesis, the closed shop was popular and, in spite of legal bans, probably still exists, in the building trades industries[3] which by necessity utilize extensive methods of production.

2. The union shop[4] and variants thereof are the more common compulsory trade union membership devices.[5] But there is nothing in the current union shop arrangements in the U.S.A. which can affect adversely managerial control over the work force. British experience cannot be applied overseas without recognizing that collective bargaining agreements in the U.S.A. are more detailed and comprehensive than in Great Britain. A typical collective bargaining agreement in the States will specify in great detail the order in which workers will be laid off in the event of a redundancy, it will lay down rules governing the method of promotions

[1] Mr. Allen's formal definition of the terms 'closed shop' and 'union shop' (pages 70 and 71 of his article) would be acceptable in the U.S.A., yet there is one, possibly important, difference in their application in the two countries. In Great Britain, as Mr. Allen points out, a closed shop agreement may or may not stipulate that a worker must be a member of a particular union, while in the U.S.A. such an agreement will invariably require membership in the one particular union which has secured bargaining rights. For an excellent exposition of the different usages of these terms in the two countries see, Jean T. McKelvey, 'The "Closed Shop" Controversy in Postwar Britain', *Industrial and Labor Relations Review*, vol. vii, No. 4, July 1954.

[2] Section 8 (*a*) (3) of the Act makes it an unfair labour practice for an employer to discriminate among employees as to hire so as to encourage membership in any labour organization. Section 8 (*b*) (3) makes it an unfair labour practice for a union to cause an employer to so discriminate.

[3] Horace E. Sheldon, *Union Security and the Taft-Hartley Act in the Buffalo Area* (Ithaca, N.Y.: New York State School of Industrial Relations, Cornell University, Aug. 1949), Research Bulletin No. 4, especially p. 41.

[4] The Taft-Hartley Act specifically permits a type of union shop provided, as Mr. Allen points out at page 79, certain conditions are met by the union. It should be noted that one of the conditions listed, that of the union security authorization election, has been repealed by an amendment to the statute (Public Law 189, approved by the President, 22 Oct. 1951).

[5] 'Union Status Provisions in Collective Agreements, 1952', *Monthly Labor Review* (Washington, D.C.: U.S. Department of Labor), vol. 76, No. 4, Apr. 1953, pp. 383–7.

and transfers and it will limit drastically the right of an employer to discharge workers.[1] It is not only the difference in the comprehensiveness of the agreements which must be taken into account but also that such agreements in the U.S.A. are binding upon all workers in the 'bargaining unit' *even if they are not members of the union.* The 'bargaining unit', a collectivity of workers to whom the terms of the agreement apply, may exist informally in Great Britain, but in the U.S.A. it has been enshrined in the basic labour law ever since the passage of the Wagner Act in 1935.[2]

In Great Britain more than one union might bargain for workers similarly situated in an industrial plant, but in the U.S.A. the idea is fundamental to the philosophy of the Wagner Act, and unchanged by the Taft-Hartley Act, that only a single bargaining representative (union) should be allowed in a bargaining unit. The corollary to this proposition is that the union chosen by the majority of the workers has the authority to represent all workers in the unit irrespective of their individual membership status. British unions may have persuaded employers that non-members should accept, within the precincts of the factory, the agreed-upon conditions of employment, but in the U.S.A. this is a matter of government compulsion. It is not the union shop which adds to or subtracts from the power of the union to interfere with management's control of the work force; if employers have lost sovereignty, it is only because of concessions made in collective bargaining on detailed specific issues.

3. Can it not be said, however, that compulsory unionism adds to a union's bargaining power and that therefore it indirectly aids in securing increased control over managerial decisions? It is not clear that Mr. Allen had this in mind but if he did, even this proposition cannot stand the test of close scrutiny under present institutional arrangements in the U.S.A. Prior to the passage of the Taft-Hartley Act, a union shop may have added to a labour organization's bargaining strength since any member who did not bend to the collective will ran the risk of expulsion and consequent loss of his job. Today, however, expulsion cannot mean loss of position unless the member was dropped for failure to tender periodic dues and fees uniformly required of all unionists in the organization.[3] A member who refused, for example, to obey a strike call might still be dropped from the rolls, but under the Taft-Hartley type of union shop, he could not therefore be dismissed from his position in the plant. Clearly this type of compulsory trade unionism, the only sort legal in the U.S.A.,

[1] Neil W. Chamberlain, *Collective Bargaining* (New York: McGraw-Hill Book Co., 1951). Chapter 14 discusses the content of the collective bargaining agreement and Appendix B is an example of such a document.

[2] Section 9 (*a*) of the Wagner Act.

[3] Sections 8 (*a*) (3), 8 (*b*) (2), and 8 (*b*) (5) of the Taft-Hartley Act.

does not add significantly to the union's bargaining strength; neither does it detract from the ability of employers to regulate the work force in an efficient manner.

Whatever the concession costs involved in granting the union shop, they cannot be measured by the loss of employer direction of the labour force. The second aspect of the thesis deals with the matter of resistance costs which, in Mr. Allen's schema, are made to depend upon the nature of the productive processes. There is something artificial in considering separately the employers' reaction to the single collective bargaining demand for some form of compulsory trade unionism. In the U.S.A., at any rate, the demand for the union shop is usually one of a list of demands served upon management at contract time and, while it is likely that emotional factors might raise the index of an employer's willingness to resist, his ability to resist because of the effects of a possible strike on the firm would seem to be entirely independent of the nature of the demand. If, however, the demand for the union shop is considered separately, it would seem that coefficients of resistance other than the degree of the intensiveness of capital use in the firm must be considered. An employer's ability to resist might also be influenced by (1) his cash position at the time of the dispute, (2) the short-term market outlook, (3) the perishability of his inventory, (4) his standing commitments with suppliers, and (5) his ability to retain custom in the event of an interruption in deliveries. These items, and many others which could be detailed,[1] might overwhelm the lone factor of the particular proportions of capital and labour utilized when the employer is pressed to calculate his resistance schedule.

Conclusions

Mr. Allen's thesis is an interesting one which may have validity for British industry but which cannot be applied to current conditions in the U.S.A. in any meaningful fashion. Managerial attitudes towards compulsory unionism in the U.S.A. cannot be distinguished according to the effects of such compulsory membership features on the efficiency of the firm. It is not the union shop which interferes with the exclusive managerial direction of the work force; it is the provisions of the comprehensive collective bargaining agreement. Such an agreement can be negotiated by the single bargaining representative which is the choice of the majority of the workers in the unit and, according to law, its terms are binding upon all employees in the unit, whether or not they are members of the labour organization. It is also probable that management's ability to resist a union's demand for compulsory unionism, like its ability to resist

[1] Cf. Chamberlain, op. cit., chapter 10.

any other collective bargaining demand, is influenced by a host of factors in addition to the one stressed by Mr. Allen.

If compulsory unionism has no independent effect on labour efficiency and if resistance is influenced by factors other than the intensity of capital use, then Mr. Allen's thesis must fail and other reasons must be sought for the endemic nature of compulsory trade unionism in the U.S.A. as compared with Great Britain. The writer of this note has no superior alternative theory to offer but suspects that an explanation of the differences would have to take into account the fact that the union shop has become an important symbol in a struggle for power between labour and management in the U.S.A.

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PROFESSOR NURKSE ON INVENTORY CYCLES

By E. S. MILLS

I

In a recent issue of this journal[1] Professor R. Nurkse investigated the 'inventory cycles' generated by two quite simple dynamic models of an economy. Although he begins with the macro structure of the economy and does not explicitly relate the models to individual decision procedures, it is instructive to consider the behaviour of individual firms which is implied by his models.

II

I shall be concerned only with the behaviour of individual firms and in order to avoid confusion with the macro variables I shall employ a rather different notation from that used by Nurkse. Using the same unit period as Nurkse, I shall assume the firm produces a single homogeneous commodity and write z_t for production in period t, x_t for sales in period t, and I_t for total inventory remaining at the end of the period. Believing, on both theoretical and empirical grounds, that the assumption of a fixed ratio of desired inventory to expected sales is unjustified, Nurkse employs instead a constant 'marginal inventory ratio'. He assumes that production in period t is equal to expected sales plus some fraction of 'passive inventory disinvestment' of the previous period, i.e.

$$z_t = x_t^e + \alpha(x_{t-1} - x_{t-1}^e),$$

where α is the marginal inventory ratio or accelerator, and x_t^e is the expected demand in period t (price assumed constant). Nurkse further assumes $x_t^e = x_{t-1}$, i.e. expectations are either single-valued or can be typified by this value on which alone inventory policy depends. We therefore have

$$z_t = x_{t-1} + \alpha(x_{t-1} - x_{t-2}),$$

which corresponds to Nurkse's equation 3 (p. 205). Clearly, in this model, production for inventory is zero whenever $x_{t-1} = x_{t-2}$, i.e. when passive inventory investment is zero. But this can occur at *any* total inventory level, and therefore not only is the ratio of inventory to sales not constant, *it is irrelevant*. This is neither correct nor consistent with Nurkse's diagram (p. 222).

Presumably, the model Nurkse has in mind is the following:

$$z_t = x_t^e + (I_t^d - I_{t-1}), \tag{1}$$

where $x_t^e = x_{t-1}$ and I_t^d, desired inventory, can be approximated over the relevant range by

$$I_t^d = k + \alpha x_t^e = k + \alpha x_{t-1}. \tag{2}$$

[1] R. Nurkse, 'Period Analysis and Inventory Cycles', *Oxford Economic Papers*, vol. vi, No. 3, pp. 203–25.

Since, by (1), production in any period is the expected level of sales plus the difference between actual and desired inventories, we have

$$I_t = I_t^d - (x_t - x_t^e). \tag{3}$$

Substituting (2) and (3) into (1), we have

$$\begin{aligned} z_t &= x_{t-1} + [(k + \alpha x_{t-1}) - (k + \alpha x_{t-2} - (x_{t-1} - x_{t-2}))] \\ &= (2+\alpha)x_{t-1} - (1+\alpha)x_{t-2}. \end{aligned} \tag{4}$$

This is a perfectly plausible and consistent model embodying the idea of a decreasing inventory-sales ratio as output increases. However, this is precisely Metzler's fourth model[1] and therefore Nurkse is wrong in thinking that it is his use of a 'marginal' accelerator which distinguishes his model from Metzler's.[2]

III

There is one other point on which I wish to comment. It is not quite clear how Nurkse wishes to apply the 'inventory control' formula presented on p. 215. Whether this formula is at all relevant to inventory theory I think is highly questionable in view of recent work on optimal inventory policy.[3] However, I shall not pursue this point here. The point at issue is how the formula is supposed to direct inventory decisions.

First of all, it is clear that the 'period' of the above model cannot refer to the total planning time (Nurkse's 'year') in the formula. If it did, the formula would refer only to intra-period inventory decisions, planned inventory at the end of the period always being zero. In this case the formula would be irrelevant to the above model since it is concerned with inventories carried over between periods.

Therefore the period in the above model can be taken to be the 'procurement period' of the formula. It then tells us the desired level of stocks for each period up to the 'horizon' (end of the 'year') as a function of expected sales, 'set-up' costs and carrying costs. It is clearly intended to be an equilibrium condition and does not specify how (or if) the firm reacts when, for example, sales are different from their expected level. However, Nurkse makes a slightly different use of the concept of set-up costs two pages later (p. 217) where he supposes them to be a linear function of the change in production between any two periods. In this case set-up costs surely affect the disequilibrium adjustment path of inventory rather than the equilibrium level as in the formula of p. 215.

[1] L. Metzler, 'Nature and Stability of Inventory Cycles', *Review of Economic Statistics*, 1941.

[2] Nurkse's second inventory accelerator—the pipeline accelerator—appears to be free from this objection.

[3] Cf. Arrow, Harris, Marschak, 'Optimal Inventory Policy', *Econometrica*, vol. xix, 1951; Dvoretsky, Kiefer, Wolfowitz, 'The Inventory Problem', I and II, *Econometrica*, vol. xx, Nos. 2 and 3.

The disequilibrium concept of set-up costs suggests a rather different model from any employed by Nurkse. It suggests that the appropriate model is

$$z_t = z_{t-1} + c(I_t^d - I_{t-1}) \quad (0 < c \leqslant 1) \tag{5}$$

where I_t^d is again given by (2). This is clearly a slightly more general model than the first one, but does not, of course, lead to a simple difference equation of the type used by Nurkse for the macro structure (unless $c = 1$).

Finally, I do not wish to imply that a theory which determined endogenously the decision period would not be a useful one. Presumably, firms make certain decisions more frequently when things are going badly than when they are going smoothly, although I have no information on the subject. What I do not see is how one is to develop such a theory from a formula such as the one used by Nurkse (p. 215).

UNIVERSITY COLLEGE OF NORTH STAFFORDSHIRE

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